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SPACE AND VISION

BY

W. H. S. MONCK, M.A.

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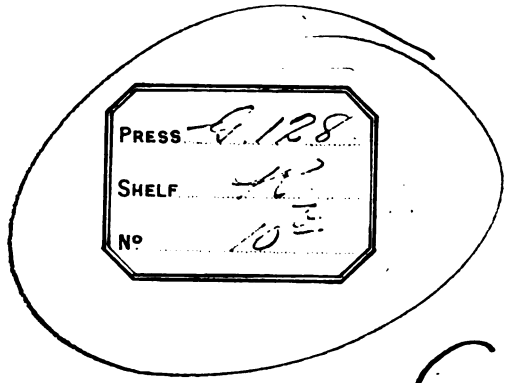

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SPACE AND VISION.

SPACE AND VISION:

AN ATTEMPT TO DEDUCE ALL OUR KNOWLEDGE OF SPACE FROM THE SENSE OF SIGHT,

WITH

A NOTE ON THE ASSOCIATION PSYCHOLOGY.

BY

W. H. S. MONCK, M.A.

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"An Essay on The Christian Miracles," &c.

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PREFACE.

THE theory advocated in the following pages first occurred to me about ten years ago, when reading the counter-theory of Brown. Its leading features were then communicated by me to one or two friends, who are still connected with the University of Dublin. The subsequent publication of Professor Abbott's work on Sight and Touch, led me to entertain my own opinions more strongly, though it will be seen that in some important respects my conclusions differ from his. The reader will see that I have borrowed several of his facts and arguments in refuting the common theory; but I think he will also recognise something of my own. As a repository of facts, indeed, Mr. Abbott's book will be indispensable to any student of the subject, however widely he may differ from the author in his explanation of them; and I venture to think that had these facts been studied with equal care by Messrs. Mill and Bain, they might have been led to cancel a good many pages of their exposition. At all events, the question has now reached a stage which renders a minute examination of the actual phenomena absolutely necessary. There are two or more theories before the world which sufficiently explain the broad facts of the case. It is by the accounts they render of the less obvious and striking phenomena that such theories must be tested. It is not in ordinary reflexion and refraction that we need expect to find an *experimentum crucis* between the

Emission and Wave Theories of Light, nor in a thunder-storm that we need look for a test of the Single and Double-fluid hypotheses in Electricity. The same observation will apply to theories in Psychology; and the writer who confines himself to the explanation of mere generalities will never establish his theory so long as there is another in the field. I should, therefore, have desired to make my own explanations more special than they are; but finding I could not devote enough time to the subject to enable me to reconsider and rewrite it completely, I have contented myself with making a few corrections and insertions in a manuscript that has been lying by me for some years. The question is one with which Trinity College, Dublin, is peculiarly identified, through Bishop Berkeley and Professor Abbott; and I trust she will not suffer it silently to fall into the hands of inquirers of other nations. Berkeley's theory of vision led directly to his now-celebrated Idealism; and if there is any mode of escaping from that subtle doctrine, I believe it must be discovered in the same field.

There is a current doctrine on this topic which is so closely related to the subject of this Essay, that I may be excused for touching briefly on it here, viz.: That tactual sensations and resistance are the great tests of material existence and reality. I find this doctrine maintained by two thinkers of such opposite schools as Messrs. Mill and Mansel. It seems to have originated in a confusion between vulgar and the philosophical conception of matter. The vulgar include in their idea cohesion of the particles, and can hardly be persuaded to regard liquids and gases as material, until they see them enclosed in some solid body which gives them a kind of artificial coherence. Philosophers took up the same idea with little examination, and thought the Newtonian theories of gravitation and inertia afforded a

strong confirmation of it. But if by matter we mean the Real in Space—that which exists therein and affects our senses—it is now certain that a great part of the material universe consists of imponderable and penetrable fluids, which would probably offer no resistance to the finest sensitive or muscular apparatus. This I take to be the proper philosophical idea of matter, and, in this sense of the term, sight is quite as competent to reveal to us the existence of matter as touch or the motor nerves. In this respect I think Kant, whose tests of material substance are permanence and action, is considerably in advance of more recent writers. Visible unresisting objects are no doubt often regarded as unreal; but this is not the case when they continue to exist permanently, and affect our senses of hearing or smell: otherwise smoke would appear as unreal as a ghost. However, if anyone desires to confine the term "matter" to tangible bodies, he is at liberty to do so. I only contend that the eye reveals to us one spacial reality, namely, the nervous organism connected with it, and that we can thence reach other spacial realities outside us by a legitimate inference. If so, whether the existence of *matter* is proved or no, Berkeley's Idealism is refuted.

I hope the concluding note on Idealistic Associationism will not be considered out of place.

SPACE AND VISION.

For some years past the leading problems of Psychology have been tending towards a single point—our idea of space and the judgments relating to it. Is the idea *a priori* or *a posteriori*?—if *a posteriori*, is it derived from sense directly, or by a slow process of combination and association?—if directly from sense, is it from sight or touch, or both?—are samples of one class of these problems. Are our spacial judgments necessary or contingent?—are they synthetical or analytical?—and if necessary, is this necessity original or derived?—are samples of another. Is the Associationist asked to produce an extreme instance of the potency of his favourite principle? He appeals, without hesitation, to the space-perceptions of sight. Is the Intellectualist asked to produce an extreme case of an original and irreversible necessity of thought? He is almost certain to appeal to the truths of Geometry. The battle-ground on which the two great psychological schools must meet and contend is thus clearly marked out; it becomes manifest that a careful investigation of the phenomena of space and vision must throw a flood of light over the whole range of the mental sciences; and he who directs the attention of earnest metaphysical inquirers to this central question may have cause to congratulate himself on having done so, though every portion of his own building should ultimately be uprooted.

I shall for the present set aside the consideration of the *a priori* origin of the idea of space. Such an assumption is only admissible if it be impossible to derive that idea, *with*

all its actual characteristics, from sense, or from sense combined and associated with other elements; and even if the *a priori* origin of the idea were granted, it is not easy to see how by means of it we could arrive at a knowledge of the particular figures, magnitudes, and situations of particular objects.* Turning, therefore, to the sensitive perception of space or extension, our attention will be, in the first place, directed to Berkeley's theory of vision. Let me remark, however, at the outset, that leaving out of account Mr. Bailey, Professor Abbott, and other avowed opponents of this theory, its professed defenders are by no means at one. This will become evident, if we recount briefly the various ideas which are popularly ascribed to sight, and observe the manner in which this rough catalogue would have been retrenched by different disciples of Berkeley. The ideas commonly referred to sight are—

1. Colour.
2. Two dimensions of space—plane superficial extension.
3. Three dimensions of space—solid figures and relief.
4. Direction from the eye.
5. Distance, and especially distance from the eye.

All Berkeleians (and indeed all philosophers) admit that the perception of colour is original to the sense of sight; and all Berkeleians deny that distance from the eye is so. But here their unity is at an end. Berkeley himself seems doubtful whether anything beside colour is originally perceived, and this doubt becomes a distinct negative in the hands of Condillac, Brown, Bain, and J. S. Mill in his Examination of Hamilton. (In his criticism of Mr. Bailey, Mr. Mill's doctrine is different.) Most Berkeleians, however, admit the originality of our visual perception of two dimen-

* This difficulty affords a satisfactory reason for Sir William Hamilton's supplementing his *a priori* origin of space by an *a posteriori* one, which has been so often stigmatized as a flagrant violation of his own law of parcimony.

sions of space; among whom I may particularly mention D'Alembert and Stewart. Many of these philosophers, indeed, virtually admit an immediate perception of trinal space, though, in consequence of confounding *superficial* with *plane* extension, they express themselves erroneously. Such, for instance, must have been the doctrine of Reid; for he maintains that persons who had no sense but that of sight, would take all objects to be portions of *spherical* surfaces. Sir William Hamilton and others have added direction from the eye and even outness, perceptions which clearly involve trinal space. Thus, while Condillac and Brown admit but one of the five present perceptions of sight to be original, Sir William Hamilton accepts four; and yet he, no less than the others, professes an almost unreserved acceptance of the Berkeleian theory.

If we inquire of these theorists, how these original deficiencies of sight are made good, we shall find an almost equal diversity of opinion. Time, perhaps, enters as an element into every solution, (even that of Berkeley himself,) but at this point the agreement ceases. One says, simple touch; another, muscular sensibility; a third, the active faculty of locomotion; and some, (as Dean Mansel, and perhaps Sir W. Hamilton,) even bring in a supposed *a priori* idea of space. Others combine two or more of these in various proportions. There is, moreover, another solution of the problem open to all who hold that colour is not the only direct perception of sight, viz., that this sense supplies its own deficiencies, by forming associations between different visual perceptions—associating, for example, the faintness and indistinctness of a seen object with the idea of a great number of intervening visible objects, which, though at present overlooked, would become visible before the object in question attained its maximum degree of brightness and distinctness. That such associations might be formed by experience, and even that they actually are formed, few persons will probably feel disposed to deny;

but I do not know whether a theorist who explained by their means the whole difference between our original and our present visual space-cognitions would be regarded as a Berkeleian.* But without taking account of this doctrine, it is evident that there is a very great difference of opinion among professed followers of Berkeley, both as regards the direct perceptions of sight and the sources of our present improved conceptions; and consequently, even if the arguments of Mr. Bailey and Professor Abbott should be totally discarded, there remains abundant reason for a new and thorough investigation of the entire problem. There is, in truth, no theory of vision which has met with exclusive or even general acceptance; but, on the contrary, there have been (and still are) a number of distinct theories advocated by different persons, and agreeing only in one, or at most in two points. If these differences have been little noticed hitherto, it is a further proof that the subject has not been studied with sufficient care. No one can seriously contend that the theory of Brown, who held that sight does not give us any idea of extension at all, is identical with that of Stewart, who contends that if the mechanism of vision were explained to a blind man, he could infer for himself the necessity of our (direct) perception of visible figure. (Dissertation, note, p. 134, Hamilton's Ed.)

Is sight, then, immediately perceptive of extension? This is the first important question which meets the inquirer, and his answer to it will possibly determine his solution of the entire problem. The *prima facie* case is certainly in favour of the affirmative. The great majority of mankind believe that they perceive extension by sight, and even chiefly by means of that sense. The analogy of the lower animals tends to the same conclusion. Their organs of touch and powers of locomotion are endlessly varied, but the vast majority of them

* He might however claim an earlier, and I think a higher, authority in his favour in Locke. That philosopher evidently holds that sight supplies its own deficiencies. Essay, book ii., cap. ix. 9.

possess two eyes situated similarly to those of the human subject. They all appear to possess a distinct knowledge of extension by sight, and exhibit it at a period so early as to lead even eminent Berkeleians to doubt whether their space-perceptions could have been formed by the complicated process of association which they insisted on in the case of man. The intimate association of colour and extension—or rather, to express the facts accurately, the belief of ordinary men that the extension which they see is coloured, and the colour which they see extended—is further evidence on the same side; and other indications might easily be added. The philosopher who maintains that we have a direct perception of extension by sight, is therefore in possession of the field, and his adversary must dislodge him before erecting his own superstructure. But the Visionist, if I may use the term, need not confine himself to the defensive. He can appeal to positive and powerful arguments in favour of his thesis. The first and strongest of these has been urged by D'Alembert and Stewart, but is, perhaps, best stated in the following passage of Sir W. Hamilton. (Lectures, ii. p. 165:—)

"It is admitted by all that we have by sight a perception of colours, consequently a perception of the difference of colours. But a perception of the distinction of colours necessarily involves the perception of a discriminating line; for if one colour be laid beside or upon another, we can only distinguish them as different by perceiving that they limit each other, which limitation necessarily affords a breadthless line—a line of demarcation. One colour laid upon another, in fact, gives a line returning upon itself, that is a figure. But a line and a figure are modifications of extension. The perception of extension, therefore, is necessarily given in the perception of colours."*

Mr. Mill, who is one of the ablest advocates of the contrary doctrine, deals with this argument in his "Examination of

* It must not be supposed, however, that we are conscious of visual extension only when and where the colours are different. If I look, for example, at a green square depicted on white paper, I see the length not only of the sides which are lines of demarcation, but of the diagonal which lies wholly within the green square.

Sir William Hamilton's Philosophy." His reply is too long to quote in extenso, but I think the purport of it is fairly given in the following extracts:—

"First, it must be observed that when the eye at present takes cognizance of a visible figure, it does not cognise it by means of colour alone, but by means of all those motions and modifications of the muscles connected with the eye, which have so great a share in giving us our acquired perceptions of sight. To determine what can be cognised by sight alone we must suppose an eye incapable of these changes. . . . Now there is nothing more certain than that an eye with its axis immovably fixed in one direction gives a full and clear vision of but a small portion of space, that to which the axis directly points, and only a faint and indistinct one of the other points surrounding it. . . This fact seems to warrant the conclusion, that if the axis of the eye were immovable, and we were without the muscular sensations which accompany and guide its movements, the impression we would have of a boundary between two colours would be so vague and indistinct as to be merely rudimentary. A rudimentary conception must be allowed. . . . But to confer on these discriminative impressions the name which denotes our matured and perfected cognition of extension, *or even to assume that they have in their nature anything in common with it*, seems to be going beyond the evidence. . . . I cannot admit that we could have what is meant by a perception of superficial space, unless we conceived it as *something which the hand could be moved across*." (Examination of Hamilton, pp. 285-7, 3rd Ed.)

He goes on to adopt Mr. Bain's statement, that "we never consider that we have a notion of space unless we distinctly recognise this possibility;" (of muscular motion;) "*but how a vision of the eye can reveal beforehand what would be the experience of the hand or the other moving members I am unable to understand.*" Further, he quotes with approbation the following remarks from a note of Mr. Bain's:—

"The *essential import* of visible form is something not attainable without the experience of moving the eye. If we looked at a little round spot, we should know an *optical* difference between it and a triangular spot, . . . but this would not be to recognise form, because by form we never mean so little as a *mere change of colour*. . . . We can never cross the chasm which separates an optical meaning from an effect combining light and movement in any other way than by bringing in experience of movement." (Examination of Hamilton, p. 291, and Bain's Senses and Intellect, pp. 366-7, 2nd edition.)

This is all the argument I can find, if I except a statement of Mr. Bain's that "our notions of form are *manifestly* obtained by working on the large scale;" not on the small space to which the field of distinct vision is confined: an assertion which needs no refutation, as it rests solely on Mr. Bain's *ipse dixit*, and appears to be opposed to all evidence.*

We may note then, as Mr. Mill's first argument, that the perception of diversities of colouring (at least to the extent requisite to give a clear idea of a bounding line or a figure) requires muscular motion as well as simple vision. This argument, however, assumes that the only way in which muscular motion contributes to the clearness and distinctness of our perception of space, is by becoming *associated* with our ideas of colour and its varieties. But it is evident, from Mr. Mill's own statement, that the muscular motions and adaptations of the eye contribute to the perfection of our visual perceptions in a very different way. The proper adjustment of the eye, and convergence of the axes, are

* I have already intimated that Mr. Mill advocates a very different doctrine in his criticism of Mr. Bailey. Indeed the following passage might almost be taken for a statement of my own theory:—"*We cannot see anything which is not painted on our retina, and we see things like or unlike according as they are painted on the retina like or unlike.* The distance between an object to our right and an object to our left is a line presented sideways, and is therefore painted on our retina *as a line*. The distance of an object from us is a line presented endways, and is represented on the retina *by a point*. It seems obvious, therefore, that we must be able by the eye alone to discriminate between unequal distances of the former kind, but not of the latter. Unequal lines drawn across our sphere of vision we can see to be unequal, because the lines which image them on the eye are also unequal. But the distances of objects from us are represented on our retina in all cases by single points, and all points being equal, all such distances must appear equal, or rather, we must be unable to see them in their character of distances at all. This argument proves that distances from us cannot be seen in the way in which we see the distances (or rather the apparent distances) of objects from one another—*namely, by the original powers of the sense of sight.*" —(Dissertations, by J. S. Mill, vol. ii. p. 95.)

necessary to *see* with distinctness the particular object looked at, and motions of the eye or the head are needed to bring the surrounding objects into the field of distinct vision. If these muscular motions contribute to the perfection of visual perceptions by altering the character of the visual sensations and perceptions themselves, the demand that the eye should be deprived of their aid when we are endeavouring to ascertain the original perceptions of that sense is manifestly unreasonable; and no one with the smallest knowledge of optics can deny that these motions affect our visual perceptions directly, as well as through the channel of association. But the truth is, that by sight we have a perception of diversity of colouring quite sufficient for the purpose of Sir W. Hamilton's argument, without introducing these muscular motions at all; and the two philosophers with whom we are arguing seem to have become indistinctly aware of this, when they admit an "optical difference," "discriminative impressions," a "rudimentary conception," &c. The fact is proved by superabundant evidence. Whenever sensations of vision are produced otherwise than by the action of light reflected from external objects, we are manifestly deprived of all these aids; and it is a well-known fact that these sensations may be produced by electricity, by a blow, by derangement of the stomach, by inflammation of the optic nerve, and other causes.* Will Mr. Mill contend that in such cases there is either no perception of extension or one dependent on the accompaniment of muscular feelings? This, however, is not all. In cases of couching for cataract adjustment and convergence are completely eliminated from our visual perceptions; and yet, as Mr. Abbott has clearly established, there are percep-

* I believe, as I shall have occasion to notice hereafter, that if philosophers would observe with care their ocular sensations when in the dark, or with their eyes shut, they would find that they are never without *some* perception of colour and visual extension. These perceptions, however, are often very faint.

tions of visible extension from the first. If the blind from cataract have any knowledge of coloured extension, (and a knowledge of coloured extension is a perfectly distinct thing from a knowledge of the form and situation *of external objects*,) they must likewise have it from vision alone. There is another experiment still more decisive. We can distinctly see the extension and even the figure of objects when illuminated by the electric spark or a flash of lightning; but it admits of demonstration that no muscular motion could take place during the time that this illumination lasts. I would add to this another fact, of which Mr. Mill is, doubtless, aware—the persistence of visual impressions. If Mr. Mill or Mr. Bain will gaze steadily on a brilliant object of any kind, and then close his eyes, he will see a similar object still, which, however, usually changes colour several times before its final disappearance. What has motion of any kind to do with this perception? The argument of Mr. Mill is, therefore, inconsistent with a number of known facts. And the reason is obvious. Small as the portion of an object is, which can be distinctly seen without muscular motion of any kind, it contains hundreds, or probably thousands, of sensitive minima, each of which is capable of being distinctly seen. The extreme minuteness of the *minimum visibile* when the eye is in good condition, and the arrangement of brilliancy and contrast of colouring favourable, is truly marvellous. The fixed stars, perhaps, afford the most remarkable example; and the effect of contrast is finely illustrated by the fact that they are rendered invisible in the day-time by an increased quantity of light—increased even in their own particular direction;* while, if

* I may here notice the prevalent mistake that the *minimum visibile* is a quantity of extension. It depends much more on the brightness of the object, and its contrast to the surrounding objects in brilliancy and colour, than on its magnitude, whether real or angular. It also depends on the state of the organ whose sensibility may be augmented as well as diminished by nervous excitement or other disorders.

we cut off the light laterally diffused by looking up a tall chimney, or from the bottom of a deep well, the stars become again visible, even at noon. The fibres of a gossamer's web are often visible at a distance of several feet.

Mr. Mill seems to have some perception of the strength of his opponent's case here, and, accordingly, he takes refuge in admitting a "rudimentary conception," or certain "discriminative impressions." Discriminative of what? Do these impressions merely discriminate colours, or do they discriminate figures and forms also? If Mr. Mill accepts the first alternative, the admission removes no part of the difficulty. Sir W. Hamilton says that a discrimination of colours necessarily involves a discrimination of lines and figures; and Mr. Mill replies, I admit the justice of the argument to this extent only, that we do, as a matter of fact, discriminate colours by unaided vision. The futility of this reply is obvious. But if Mr. Mill concedes that these impressions discriminate figures and forms he concedes the whole question at issue. From the whole tone of the passage, it would appear as if Mr. Mill desired to admit more than a discrimination of colours and less than a discrimination of figures and forms. But what is the third idea intermediate between colour and visible figure which is thus referred to sight? Has any one ever experienced it? Has it got a name? Or is there any evidence whatever of its existence? Moreover, if Mr. Mill means to concede the existence of this hitherto unobserved *tertium quid*, Mr. Bain does not. He seems expressly to identify "change of colour" with all observable "optical differences" in objects. The expression, "*change* of colour," I may remark, in the first place, is inaccurate. There is no change—no succession—of colours in our perception of *variety*, but the two colours are perceived at the same time as distinct from and limited by each other. Waiving this objection, when Mr. Bain identifies "change of colour" with "optical difference," he assumes that diversity of colour exists by itself

alone, and has no necessary connection with visible figure and form. He forgets that his antagonist has advanced forcible argument to prove that such a necessary connection exists and takes the very unphilosophical liberty of assuming the contrary, and leaving the argument in question unanswered.

The second part of Messrs. Mill and Bain's reply appears to me to consist in defining space so as to include muscular motion in the definition, and then arguing that because mere sight cannot give us the idea of muscular motion, it cannot give us any direct perception of space. The question then would appear to turn on the correctness of this definition; but it must be remarked, that even were it correct, it would not follow that sight could not give us all the other elements of the idea of space, or that these other elements do not constitute the greater and more characteristic part of it; and if this were granted I believe Sir William Hamilton would have gained everything worth contending for. Mr. Mill maintains that nothing can be properly designated space unless we conceive our hand capable of being moved across it; and although Mr. Bain is more liberal and allows that a motion of some other part of our bodies might answer equally well, he too includes muscular motion of some kind in his definition. I think I can here fairly appeal to the reader whether he is conscious of any such element in the idea which he attaches to the term? requesting him to bear in mind that the question is not, whether the idea of motion or mobility is intimately associated with that of space, (which from the nature of the case it must be,) but whether the former idea forms a constituent part of the latter? But even supposing this question decided against me, my general thesis is by no means overthrown. We have a *visual* idea of motion,* and it is very doubtful if we have any other. It will, I think, appear highly probable from what will be said hereafter, that a man destitute of the sense of

* We *see* two objects approaching each other without any change in our own position. This, I think, may be fairly called visible motion. It is quite distinct from muscular motion, (or rather muscular *action*,) which can only be perceived in ourselves and not in external objects.

sight would perceive his voluntary motions only in their character of voluntary efforts or muscular actions. At all events, the idea of motion derived from touch or the muscular sense is not the only one we possess; and supposing motion or mobility to be a necessary ingredient in our idea of space, the question arises whether this motion be not visible motion? And here I appeal to the consciousness of my readers with much greater confidence; for in my mind at least it is visible motion which is intimately associated with, or forms an element of my idea of space. And I think I can appeal to Mr. Mill himself in corroboration of this truth; for he unequivocally admits that our present pictures of spaces and extensions are exclusively "eye-pictures." "The visual imagery," he says, "effaces from our minds any distinct consciousness of the series of muscular sensations, of which it has become representative. The simultaneous visual sensations are to us symbols of tactual and muscular ones, which were slowly successive;" (Examination of Hamilton, p. 283;) and he goes on to adopt a paragraph from Mr. Herbert Spencer, in which the latter says that "these symbols occupy the mind to the entire exclusion of the ideas symbolised." (Ibid.) It is thus conceded that if there is any representation of motion in this picture of space, it must be a representation of visible motion; and indeed there is a further concession here made that when we employ spacial terms the sole object of consciousness is generally, if not always, a collection of visual sensations. Messrs. Mill and Spencer, indeed, speak of these visual sensations as merely the symbols of other ideas, seldom or never realised, but which nevertheless constitute our true notion of space. Now, in the first place, it appears to me that the idea which every one has in his mind when he employs spacial terms, is the true idea of space:* and in the second

* Mr. Mill, after observing that the blind man's idea of space is sometimes wanting in the element of the simultaneity of the parts, proceeds to confess that "what is thus wanting to him is the *chief feature* of the conception as it is in us." (Exam. of Hamilton, p. 278.) If anything is essential to a conception one would imagine the "chief feature" of it is so.

place these visual ideas are themselves intuitive cognitions, and if the philosophical phraseology of Messrs. Mill, Bain, and Spencer will not permit us to apply our ordinary space-terms to them, their existence and importance ought to be admitted, and they ought to receive proper designations of their own. I may add, that in the idea of space described by Messrs. Mill and Bain, an element which I believe most persons will think much more important than that of mobility, namely, the simultaneous existence of its parts is suffered almost entirely to evaporate. As regards the prophetic power supposed to be ascribed to the sense of sight by the Visionist, it has, I think, never been claimed by any advocate of that theory; unless it merely means that nothing can change its situation from one place to another at a distance from it without passing through an uninterrupted series of intervening positions—a proposition whose self-evidence will, I think, be generally admitted.

When Mr. Bain says that the "essential import" of visible form is something not attainable without experience of motion, he overlooks the fact that the principal use of a thing is not the thing itself. The principal use of the sense of hearing is to assist us in holding communication with our fellow-creatures; but no one would on this account propose to limit the meaning of the word "sound" to articulate speech, and deny that the "essential import" of the perceptions of the ear could be known to one who had never heard men speak, but who had heard other noises. Besides, if the guidance of muscular motion is one of the chief uses of our visual space-perceptions, it is by no means the only one. In reading a book, for example—and few uses of sight are more important than this—muscular motion plays a very subordinate part, and much the greater portion of the employment of the sense consists in the simple perception of visible figure. I am therefore unable to see any ground on which Messrs. Mill and Bain's objection to Hamilton's argument can be sustained.

This, however, is not the only argument in favour of the perception of extension by sight. Let us suppose sight incapable of presenting to us anything besides colour, and inquire what means we have for ascertaining the extension and figure of external objects on this supposition? Touch and locomotion; of which modern writers lay most stress on the latter. But it must be noted, that unless muscular motion be guided by tactual differences, it can give us no information of the figure and form *of objects*. This is evident; for one object can only be discerned from another by sight or by touch; and sight, (on our present hypothesis,) presenting diversities of colour only, can afford no guidance to the hand (or other member) in moving round an object which presents no tangible peculiarities to distinguish it from those that surround it. Hence we find a limit to the association of visual impressions with tactual and muscular ones. If, in the great majority of the objects which come within the reach of a child's senses, diversities of colour are attended with tactual differences, the association will become very close and intimate; if, on the other hand, most of the objects present exhibit differences of colour without any corresponding diversities of tactual sensations, the association between these impressions will be very feeble, and diversities of colour will suggest diversities of form either very slightly or not at all. Now, let us take a child who is born in the higher ranks of life, and inquire how the fact stands in his case? In how many of the objects which are presented to him are diversities of colouring attended by any other perceptible diversity? Almost every article of clothing which he sees, (including the carpet, the table-cover, and all similar objects,) the room-paper, pictures, books, newspapers, and even the looking-glass when he looks at it, present varieties of colouring where there are no tangible differences, and where, if sight merely presents us with colour, there is nothing whatever to guide the hand in ascertaining the shape. Years roll on. Reared amid these

sights, our child becomes a student, who spends the greater part of his waking hours in deciphering letters in various languages, not one of which presents any tangible peculiarity, and yet the very meaning of a word or a passage often depends on a very slight difference in the shape of a letter—a difference too minute to distinguish clearly by touch or locomotion, even if there was anything to be felt along its outlines; and it may be added, that the clearest perception of the figure and form of these objects coexists with the fullest conviction that touch and locomotion would find no distinguishable characteristics in them. What becomes of the Association theory here? It would, indeed, be almost a sufficient refutation of it to say, (as Mr. Abbott does,) that we do not handle one object out of a thousand we see, and that consequently we cannot form by this means an association so intimate that figure is always and necessarily suggested to us by diversity of colour. When we add, that objects too minute to be distinctly felt nevertheless give us the clearest impressions of their figure and magnitude, the argument is much strengthened; but when to this we can still add, that objects large enough to exercise our tactual and muscular senses, and on which we do exercise them, do not present to these senses any distinguishable trait whatever; and that, notwithstanding this, we recognise their figure by sight with a clearness and accuracy to which unaided touch and locomotion never approximate—and this not merely now and then, but at every instant that the diversities of colour are presented to us—I think we have a second and hardly less powerful argument in favour of the direct perception of extension by sight.

These two arguments are, I think, sufficient, and if they were not they might be added to.* But what is it that

* It may not, perhaps, be amiss to state an argument virtually the same with Sir W. Hamilton's in a different form. If colour be a mere sensation, presentations of a single colour can differ only in intensity,

sight informs us of the extension of? Does it give us (as the vulgar seem to think) the real magnitude and figure of external objects? Or, does it give us their projection on a plane? Or, does it give us their projection on the sensitive surface of the eye—the well-known picture on the retina?

There is, I think, abundant proof that we do not see the real external objects. Every illusion of sight is evidence of the fact; and these illusions are of such a character as, under certain circumstances, to reverse every spacial relation of these objects. Reflexion, refraction, and aberration alter the apparent direction of external objects from us, and so does the time-propagation of light. The latter in particular is worth attending to, as in the case of a fixed star the real direction of the object may be directly the reverse of that in which it is seen, and the star may even have ceased to exist thousands of years ago, and is nevertheless still perceived. The shape of the external object is altered by refraction and perspective. It may be converted into two or more objects by using a multiplying glass, by a proper direction of the axes of the eyes, or by placing it close to the eye and looking at it through a number of pin-holes. Paintings and the stereoscope afford sufficient evidence that

and presentations of two simultaneous colours can differ only in the absolute and relative intensity of the two. Hence, when we see two colours at once, (red and blue suppose,) the impression received depends only on the quantity of each kind of light that falls on the retina, and the sensitiveness of the place on which it falls; and consequently if we take care to make the resultant or joint effect of these two factors constant, we may vary the distribution of the two colours in any way we please without altering the resulting sensation. This is contrary to the fact. Hence we conclude that there is a perceptible difference in our sensations of colours quite distinct from their absolute or relative intensity; and as this is not a difference in intensive quantity, those who deny it to be a difference in extensive quantity (or rather in spacial relations) are bound to tell us what it is. To call it a "discriminative impression" is to throw no light on its character. The real question is, what does it (originally) discriminate?

the dimensions of an object can be altered; and every one who has looked through both ends of a telescope knows that an object can easily be made to appear farther off or nearer. This latter effect can also be produced by looking at any uniformly-coloured objects, such as clouds or water, through a long narrow tube. Objects, moreover, continue to be seen after the light from them has ceased to impinge upon the eye, and visual extension is often perceived when no objects are present, and even when the eye is closed. The variations in the apparent magnitude of external objects produced by distance and other circumstances, are known to every one. If it be objected, that in some of these cases the medium is artificially modified by glasses, &c., the reply is that what we see is not the sum of the objects and the glasses, but an illusive representation of the objects. External objects, therefore, cannot be immediately perceived by sight unless we deny the veracity of direct consciousness.

A doctrine not very dissimilar to the vulgar error just refuted is advocated by Mr. Abbott in his able work on "Sight and Touch." This author maintains, that distance from the eye and relief are original perceptions of sight, dependent on certain visual sensations connected with convergence and adjustment, and so completely determined by these sensations, that even when the latter occur without the presence of external objects, the corresponding perceptions follow. That this is no misrepresentation of the doctrine alluded to is evident from the fact, that Mr. Abbott quotes in confirmation of it Funke's assertion that "we place without us in space not only the entoptic appearances in the open eye, but also the subjective appearances in the closed eye;" (Sight and Touch, p. 79;) and from his insisting that if we *at first* saw objects in contact with (or in) the eye, "erect vision as well as single vision is simply inexplicable." (Id. p. 78.) Now, I cannot see any difference between an original law or natural suggestion which leads us into error in certain cases, (and this not by consequence but

directly,) and a consciousness whose direct deliverance is sometimes untrue; and, consequently, if Philosophy should ever adopt Mr. Abbott's theory that a subjective appearance in the closed eye is immediately perceived as external, (and not judged to be so in consequence of early association,) she will thereby commit suicide. But I do not think that the facts to which Mr. Abbott appeals justify such a conclusion. They will be considered hereafter.

Nor can I see any valid reason for believing that objects are originally seen as if projected upon a plane. The only reason to think so is that the appearance of solidity can be imitated by a plane figure, as in a picture or the stereoscope; but I think there is no doubt that a figure of three dimensions could be equally made to represent a plane, as indeed actually occurs in the case of the sun and moon. Neither the external object nor the sensitive surface of the retina are planes, and consequently the supposed plane of projection does not exist anywhere. How then are we to reconcile the theory of plane-perception with the veracity of consciousness? The chief source of this doctrine seems to be a confusion of plane with superficial extension. Philosophers, however, have by no means uniformly adopted it. Reid, in his chapter on the Geometry of Visibles, maintains that in immediate vision all objects appear as portions of spheres, and proceeds to show how completely Geometry would be metamorphosed in the case of persons possessing the sense of sight only. In fact, if we prolong to infinity the pencil of rays, proceeding from any distant object to the eye, and make a section of it at a sufficient distance from the eye to render the effects of convergence and adjustment insensible, it is evident that this section may be substituted for the real source of light without any change in the resulting visual impression; but as any one of an infinite number of sections will produce the same effect, we cannot say that this effect is that of a plane or of a spherical section rather than that of any other we choose to name. Mr. Abbott, too, mentions facts which seem to me

quite inconsistent with the plane-theory, as I shall have occasion to notice hereafter. I may further observe, that Reid's doctrine, by apparently denying the necessity of geometrical truths, proves so attractive to Mr. Mill that he accepts it completely, (Examination of Hamilton, p. 87,) forgetting that he is about to advocate a doctrine which confines the sense of sight to the perception of colour, and of course excludes it from perceiving either plane or spherical extension.

This is, perhaps, the best place to discuss the question, whether sight is or is not originally perceptive of distance, or rather of distance *from the eye*. Against this doctrine the chief arguments of Berkeley were directed, and the passage already quoted from Mr. Mill's criticism on Mr. Bailey states one of them in a clear and precise form. The argument in question rests on the facts that lateral distance is represented on the retina, and that longitudinal distance, or distance from the eye, is not; while in all other respects the perception is known to correspond with the picture on the retina. This argument seems to me a powerful one, but Berkeley could not state it with sufficient clearness, because he evidently hesitated about allowing to the eye any perception of lateral distance; while Mr. Mill, by his later adoption of the views of Brown and Bain, precludes himself from employing it at all. There are, however, many other reasons for denying this perception of distance from the eye. Distance is always measured between *two* things, and hence to see distance from the eye, we must see *both* the eye and the object. If I do not know where my eye is, I cannot know how far a visible object is distant from it; and if I do not see the external object, I cannot know how far it is from my eye. Now, I have already adduced reasons for holding that the immediate object of sight is not anything external to us; but in addition to this, those who hold that we see the external objects themselves, are positive that we do not see the eye. This statement is clearly made by Mr. Abbott:—"Sight

can plainly give only a vague and negative notion of the place of the seeing organ." (Sight and Touch, p. 77.) Nor can touch and locomotion remedy this defect; for, "in the first place, it is only by rare accident that we touch the eye-ball; the sensitive organ which is not external cannot be touched at all; and, secondly, touching either the eye-lid or the eye produces no visual sensation. The hand, then, cannot by contact produce vision, but it can when not in contact, interrupt the sensation;" and, consequently, "touch cannot discover to us the place of the eye, as it does that of the tactile surface of the body—namely, by compelling us to associate a certain part as felt with a certain organ, as feeling." (Ibid.) The organ, he adds, moreover, does not, properly speaking, *feel* at all; there is no consciousness of the *locality* of the sensorial affection; of which we have a "demonstrative proof" in the fact that, "notwithstanding all our experience, we cannot with the utmost attention distinguish the impressions on one retina from those on the other." (Id. p. 78.)* Mr. Abbott seems quite unconscious of the havoc which these admissions make in his own system. Indeed he goes on to maintain that this perception of distance from the eye takes place even when the object is in the eye, nay, actually on the retina; for in proof of his doctrine he quotes Funke, who says that subjective appearances in the closed eye naturally appear to be in space without us, "and this to such a degree, that even when the subjective image of a flame is observed in the closed eye we involuntarily form a judgment of its distance." (Sight and Touch, p. 79.) But neither his denial that we have any perception of the

* On the same rock the doctrine of Sir W. Hamilton, that we are directly conscious of the *outness* and *direction* of the objects of vision, is shipwrecked. To know that a thing is in a particular direction, I must know the position of the thing from which this direction is measured, and to know that A is outside B, I must know that B is outside A. If I do not perceive B, I cannot perceive the direction of A from B, nor even know that A is outside B at all.

position of the eye—from which, nevertheless, we measure not only directions but distances—nor the consequence that we see in direct and original vision the distance of the eye from objects that are in it, seem in the least to disconcert Mr. Abbott. He endeavours to prove that we have no perception of distance by any other means than vision, which I concede. He overlooks altogether the fact that vision can educate itself, though quoting passages from other authors* which establish this truth; and he then proceeds to determine the physical antecedents of the perception of distance, as if there was no doubt that the perception was an original one. But these antecedents, I think, afford a new argument against his doctrine.

Mr. Abbott's antecedents are the visual sensations which (in his opinion) determine the convergence and adjustment of the eyes, and which he thinks consist in the relative brightness, diffusion, and multiplicity of the pictures on the central and lateral parts of the retina, together with the duplicity and separation of the images in binocular vision. Now, in the first place, it is evident that these antecedents consist of a large number of sensations, and the resulting perception supposes a comparison between them. This is admitted by Mr. Abbott himself, who concedes the necessity of a varied lateral field, and gives a good instance of its necessity in the apparent nearness of the stars, as seen by the sweeper of a tall chimney. This already looks sufficiently like association, and unlike direct perception. But let us next observe, that all these sensations vanish as soon as the proper convergence and adjustment are attained, or rather that they are replaced by different sensations of the same description, which must, upon Mr. Abbott's theory, suggest a different distance. Our perception of distance will therefore be accurate only for the moment that the eye is first turned on the object, and will cease to be so if we continue to look at it. But even this is not all: for these sensations of the first

* See Sight and Touch, p. 38.

instant which, according to Mr. Abbott, determine the correct adjustment and convergence, as well as our perception of distance from the eye, are themselves not invariable when the distance is the same, but depend also upon the previous convergence and adjustment. Nor would Mr. Abbott come off much better if he had said (which he does not) that the sensations which follow the proper adjustment and convergence determine our perception of distance; while if he called on both classes of sensations the perception of distance would result from observation of a series of *successive* phenomena, and therefore could not upon his own showing be an original one.

Mr. Abbott holds that longitudinal distance is a perception equally direct and immediate with lateral: yet he makes a comparison of many sensations a necessary antecedent of the former. This comparison again is made between the pictures corresponding to the central and lateral parts of the retina with respect to their relative brightness, distinctness, &c. Now, in the first places, to observe the *rapidity** of this loss of distinctness as we proceed outwards from the centre, it is necessary not only to observe the actual brightness or diffusion of the various images, but also their lateral distances; and thus the perception of lateral distance becomes, on Mr. Abbott's own showing, a necessary antecedent to the perception of longitudinal. Further, how are we to distinguish between the central and lateral parts of the retina? There is no absolute line of demarcation, and consequently this distinction itself presupposes the perception of lateral distance, since by this means alone is the distinction in question rendered possible. This presupposition of many sensations,

* "Lateral objects," says Mr. Abbott, "supply a sort of base from which distances are measured; this base including, as its most important part, the body itself, whether actually seen or not. In such estimation of distance, it is clearly of some consequence whether the loss of distinctness in the parts near the centre of the field is slow or rapid." (Sight and Touch, p. 99. See also p. 125.)

a comparison between them and a perception of their relative and absolute lateral distances, is anything but favorable to the theory of direct perception; but in the case of the binocular perception of relief, (which is clearly a perception of the same description with that of distance,) Mr. Abbott is moreover at pains to point out that the conditioning or antecedent impressions on the two eyes need not be simultaneous—a fact, which to my mind is perfectly subversive of the theory of direct perception of distance from the eye.

In arguing that the perceptions of distance and relief are original, Mr. Abbott lays great stress on binocular vision, and regards the stereoscope as an *experimentum crucis* of the correctness of his theory. (See in particular Sight and Touch, p. 117.) But it must be recollected that in the stereoscope the relief is not real but illusory, and that the instrument is constructed with the object of making the illusion as striking as possible. In the perception of real relief, the difference between single and binocular vision appears to be trifling, and this trifling difference is easily accounted for by association of visual impressions. In ordinary vision we are quite unaware how much of the landscape is seen by two eyes and how much by one eye only—a clear proof that there is no abrupt change as we pass from the one to the other. Instances, too, are recorded of men who have lost the sight of one eye without knowing it, which would be impossible if this loss involved that of so remarkable a perception as relief. Relief can be realised to a single eye to a very considerable extent in paintings; and perhaps, if painters were willing to sacrifice the background entirely, they might bring the foreground into stronger relief. But we have a demonstrative proof that this perception of one-eyed relief is not original in the case of Cheselden's patient, since he did not perceive it for two months after he was couched. Mr. Abbott thinks that a certain perception of distance and relief is original, even in vision with one eye: chiefly because an object A presents different appearances to the eye when it is

placed at two different distances, *a* and *b*, its direction from the eye remaining unaltered. And this is true: but it appears from Mr. Abbott's own plates, (Sight and Touch, p. 90,) that an object B at the distance *a*, will produce the same impression as the object A at the distance *b*. This seems to me to show that we can have no immediate perception of the distance of either object from the eye.

Another fact, too, seems to me to go far towards proving that our perception of relief is not original—that a figure can be seen at pleasure either as of two or of three dimensions, and that a plane figure can be made to appear as of three dimensions without any aid from colouring, brightness, confusion, &c., whatever. Every one who has studied Geometry of Three Dimensions is aware of this fact. Three lines for instance are drawn upon a plane, no two of which are mutually at right angles, and without any artificial assistance whatever they are regarded throughout some long problem as three rectangular lines in space: and it often happens that we can at pleasure see all three lines in the plane of the paper, or any two of them in that plane, and the third at right angles to it. The fact is a remarkable one, and deserves more attention than it has hitherto received. I would explain it by saying that the ideas suggestive of (geometrical) solidity are here borrowed from imagination, (by an act of will,) and not (as usually happens) directly perceived: but however explained, it seems difficult to reconcile it with the doctrine that relief is an immediate visual perception.

The facts relative to the space-perceptions of the lower animals, &c., on which Mr. Abbott places a good deal of reliance, are, I think, very little to his purpose. What he has to prove is an immediate or direct perception of distance *from the eye*; but the instances in question only tend to prove a perception of distance from some other member of the body—*e.g.*, from the beak of a bird or the claws and mouth of a quadruped. Animals never grasp anything with the eye, and when they wish to avoid contact between the eye and an external object,

it is the distance of the object from the anterior surface of cornea and not from the real visual organ which it concerns them to know. If the animal originally perceived the distance of objects from the retinal surface, this knowledge would not of itself be of any use, and before it could apply it to any practical purpose it would have to solve an intricate problem in Natural Geometry. Then what are we to say of binocular vision? From which eye is distance originally perceived? Mr. Abbott replies from neither, but from a point situated between the two, which he terms "the binocular centre." (Sight and Touch, p. 109.) A perception of distance or direction from this point would be of still less utility, for it would not enable us to ward off an external object from either cornea. Let us add, that the nose is always imaged (though not very distinctly) on both retinæ, and hence this perception of distance from the binocular centre may be resolved into a corrected estimation of lateral distance. Mr. Abbott further holds, that when we see a difference of distance between two objects not in the same direction, "the distance thus measured or perceived is not the difference of distance from the eye as centre, but the projection of this on the line of sight." (Sight and Touch, p. 99.) This knowledge if real (and it appears to me that this doctrine is contrary to all present experience) would be of still less use in guiding our voluntary motions. I have already quoted his remark that lateral objects, and especially our own bodies (whether actually seen or not) afford a kind of base from which the distances of objects are measured. What need of such a base if distance from the eye is a direct perception of sight?

On the whole, I cannot see that Mr. Abbott has advanced far in proving that longitudinal distance and relief (as well as externality and direction) are original perceptions of vision. He has advanced arguments, which I think of great weight to prove that if these were not original perceptions of vision, touch and locomotion could not *directly* supply their place; (the theory of Mill and Bain which denies space to be a

direct perception at all, and regards it as a compound made out of many feelings by association, he seems scarcely to have contemplated;) and he has brought forward reasons which are not to be despised, to prove that the signs which suggest to us longitudinal distance are not faintness, indistinctness, and the muscular sensations connected with the eye, &c., but certain other visual sensations which determine convergence and adjustment. But the first part of this argument neither refutes those who hold that lateral visual impressions become associated and correct each other, nor even Mr. Mill: while, in the second part, Mr. Abbott has merely substituted one set of suggesting ideas for another. That the connexion between these visual sensations which determine convergence and adjustment, and the judgment of longitudinal distance which follows them is anything more than one of intimate association and ready suggestion, Mr. Abbott has hardly attempted to prove.*

If, then, the object of vision be nothing external to the eye, the natural conclusion is that it is the picture, or collection of pictures, on the retina, the exact conformity of which to the accompanying visual impression has been so frequently remarked. This doctrine will at first sight appear no novelty. Mr. Mill, in a passage already quoted from his Dissertations and Discussions, implicitly adopts it. Reid, in his Geometry of Visibles, seems to entertain the same view. Dr. Nunneley says, "In seeing, it is not the tangible and large distant landscape, but the very different and small picture thrown on the retina itself, that is the immediate and only object of perception." (Nunneley on the Organs of Vision, p. 7; see also p. 20.) Brown represents this as the doctrine of all Berkeleians prior to his time, (Lects. xxviii., xxix.,) and Dean Mansel tells us that in certain cases we can

* The theory of the line of visible direction is exposed to all of the foregoing objections which apply to direction as well as distance, and seems moreover to be inapplicable to the lower animals, whose retinæ not being spherical, perpendiculars to their surfaces do not pass through a point.

even still bring the question to the test of direct experience, the sensation of darkness being merely the consciousness of the retina in a state of repose; (Metaphysics, p. 78;) and still more decisively, "The presented object is on the surface of the retina." (Metaphysics, p. 80.) It would be easy to make further quotations to the same effect. Yet, paradoxical as it may appear, the doctrine in question has never, I believe, been really maintained. Mr. Mill, as early as the Essay I alluded to, adopted almost in its entirety the hostile theory of Brown. Reid stopped short at the sphericity of the presented objects. Dr. Nunneley, speaking of the variations of brightness, distinctness, &c., in the images on the retina, says—"Not only is the mind unconscious of these variations, but it is in point of fact unaware of the existence of the figure itself, much less of differences in it;" (Organs of Vision, p. 27;) and Dean Mansel observes, that "when theory declares that the object we really see is in the organ of sight, it does not follow that the infant has ever actually seen it there." (Metaphysics, p. 117.) Sir W. Hamilton and M. Cousin, I may notice, are no less than Messrs. Bailey and Abbott openly opposed to this doctrine; and a similar remark will, of course, apply to Messrs. Brown, Mill, and Bain.*

The mere existence of this picture is to me a strong argument in favour of its being the immediate object of visual perception, because it does not appear to answer any other purpose; and this argument, of course, becomes stronger as the arrangements for producing the picture are more universally met with, more complicated, and more minutely accurate. Let any one, for example, take up the description of our visual apparatus in Paley's Natural Theology, (which is

* Berkeley himself expressly denies that the pictures on the retina are the objects of sight. (Theory of Vision Vindicated and Explained, Sect. l.) It will be seen how widely this differs from the passage already quoted from Mr. Mill's answer to Mr. Samuel Bailey, in which the former believes that he is vindicating the Berkeleian doctrine.

there given as a proof of design in nature,) and ask himself at the end of it whether he can seriously believe that Nature (or the God of Nature) took all this trouble to produce an invisible picture of a visible object? But while this picture serves no other end, (and ends are guiding principles in all our physiological researches,) it is confessed to be the universal and necessary condition of vision.* Further, we have seen, on the one hand, that if we make any other spacial object the immediate object of vision, we are almost necessarily led to hold that consciousness is in some instances deceptive; while, on the other hand, consciousness appears to indicate clearly that the immediate object of vision *is* spacial: and hence we are almost driven to believe in the immediate perception of the retinal picture, in order to save the veracity of immediate consciousness. But the strongest evidence on the subject is afforded by the exact correspondence between the picture and the accompanying or resulting visual perception—a correspondence so precise and accurate, that the optician never doubts for a moment that the same change will take place in the vision of his patient, that he proposes to produce in the picture by means of his glasses. He only inquires what kind of glass will render the picture distinct, and when that is done he is satisfied. Conclusions, too, as to the nature of the lenses of the eye as optical instruments, are constantly drawn from the facts of vision without the slightest hesitation. It was, indeed, the facts of vision that first suggested to Dollond the falsity of Newton's conclusion as to the impossibility of an achromatic combination; and it has since been concluded that these lenses are not perfectly achromatic, in consequence of the discovery of some small symptoms of chromatic aberration in the visual phenomena. No writer

* In certain cases, however, the retinal affection cannot be strictly called a *picture*, as it represents nothing without us, but is determined by internal (though not purely mental) causes. The retinal affection, however, could even here be represented in a picture or drawing, and may, therefore, receive the same name.

on Optics, in fact, imagines that he is only describing the unseen retinal picture, and not the real object of visual perception, when he explains, from the laws of reflection and refraction, why a particular external object must be seen in a particular direction, situation, &c. Objects disappearing while within the limits of the visual field are considered to afford full proof of the existence of insensible points on the retina, and even to determine their precise position. The correspondence in all these cases is assumed to be exact, and all experience justifies the assumption. It is, moreover, worthy of remark, that this correspondence is not supposed to exist between the pictures on the retina and our first uneducated visual impressions, but between these pictures and our present educated vision—a fact which would seem to indicate that association has much less influence on our present perceptions of sight than is commonly supposed.

Entoptic appearances and visual phenomena in the closed eye strongly bear out the same conclusion; and I think that if carefully attended to, many more phenomena of this class would be observed than have been hitherto. It is hardly necessary to remark, that no one suspected that vision was affected by the blood-vessels of the retina, till the fact was brought to light by the observations (they can hardly be called experiments) of Purkinje. I believe that these blood-vessels are also perpetually visible under other circumstances, which, perhaps, are now noticed for the first time. When I look at any not very brilliant and pretty uniformly-coloured object, I can almost always see some of them on its surface traced in a peculiar manner, somewhat resembling water-marks on paper. Thus seen, they seldom exhibit any trace of red colour. They will, perhaps, be best seen soon after waking in the morning, when the eye is fresh, on looking at a cloudy sky; but I have sometimes seen them under very different circumstances—sometimes even projected against the side of my nose. They seem quite as far off as the object looked at. On nearly closing

the eye, so as to make the outlines of external objects more or less indistinct, I can frequently see the field of vision covered with a kind of minute-net-work, with roundish meshes apparently corresponding to the cones and bulbs of Kolliker. The conditions most favourable for seeing these appear to be the same as for Purkinje's blood-vessels, which are likewise often rendered more visible by a partial closing of the eye. There is also a collection of brilliant round spots about the centre of the visual field, which, though perhaps not always capable of being rendered visible, very frequently are so. They can be seen in the dusk, or in a darkened room, projected against the objects looked at; and they are also often visible with the eyes shut, which does not seem to be the case with the phenomena hitherto noticed. I have made further observations with my eyes shut, with results which surprised me. At first, probably from habitual inattention, I could only say of the visual perception with light excluded from the eye, that it was a spacial one, and not uniformly black, several ill-defined grayish or slightly luminous spots appearing in it. But I now never close my eyes for a minute without a distinct perception of colours, resting, of course, on a dark ground. Blue was the first I noticed, and I have often since observed green and red—very rarely yellow, but this perhaps arises from some peculiarity in my own eyes. They generally appear in moving patches, not well defined, but sometimes by no means wanting in brilliancy. As I never observed these before I paid special attention to the subject, and have frequently seen them when in perfect health, they are evidently different from the vision of colours produced by derangement of the stomach or other disorders, and already well known. Persons accustomed to make delicate experiments in Optics are well aware that subjective influences in vision extend much beyond what is generally supposed. There are experiments of importance which cannot be made after long fasting, after a hearty dinner, or after drinking a very moderate quantity of ardent

spirits. A remarkable instance of the effects of bodily health, and especially the state of the nervous system, (of which the retina forms a part,) on our visual perceptions, occurs in a letter of Shelley's, written at the time that he was composing his famous Prometheus Vinctus: — 'My health," he says, "has been materially worse. My feelings at intervals are of a deadly and torpid kind, or awakened to such a state of unnatural and keen excitement, that only to instance the organ of sight, I find the very blades of grass and the boughs of distant trees present themselves to me with microscopic distinctness." It deserves to be noticed (though the observation can hardly be called an entoptic one) that most of the phenomena of diffraction can be produced, under favourable circumstances, by the hairs of the eye-lash. This proves that the interference of the luminous waves on the retina produces the common visual effects of interference. There are, again, some very remarkable differences between interference experiments, when the light is received on a screen, and when it is received directly on the eye, which are easily explicable if the pictures on the retina are the objects of vision, but which appear otherwise wholly unaccountable.

A host of objections to the doctrine that the pictures on the retina are the immediate and only objects of visual perception will at once occur to the reader. First, perhaps, it will be said that we are not at all aware of the existence of such a picture until science discovers it to us at a late period of life. But I apprehend that the true state of the case is merely that we are until then ignorant of the *locality* of this collection of pictures—which on my principles we must be. For of the tangible parts of the head which surround these pictures we see nothing, and their existence and locality is not, therefore, revealed to us by direct consciousness. We see nothing and perceive nothing in space, except the collection of pictures on our retina, and for this very reason we can have no direct perception of

their locality or spacial relations to anything else. And that this is the true explanation of the fact is strongly confirmed by some cases of couching for cataract. Thus, Cheselden's patient "was never able to imagine any lines beyond the bounds he saw. The room he was in, he said, he knew to be part of the house, yet he could not conceive that the whole house could look bigger." (See Theory of Vision Vindicated and Explained, sec. lxxi.) If space is originally presented *only* as a collection of retinal pictures, our inability to conceive spacial objects outside of this collection (and, consequently, to determine the position in space of this collection) needs no explanation.

The illusions of sight will, perhaps, be adduced as affording further evidence against the view here advocated. But on this theory these illusions become at once explicable. We have, in the first place, the large invariable external object, and, in the second, the much smaller variable picture of it; and all illusions arise from attributing to one of these the properties of the other. Mr. Abbott has clearly shown that touch is subject to similar illusions. This is particularly remarkable in the case of tactual magnitude. In Weber's experiments two compass points were pressed on the skin at short distances from each other, and moved apart until separate impressions were received from them. It was found that at different parts of the body the distances of these points varied in the ratio of sixty to one, and the intervals were felt to be greatest where they were really least. Illusions with respect to shape also occur in this sense. But how are these illusions possible, if the modes of extension are original perceptions of touch? The impressing object and the impressed organ have the same extension and the same figure, and, therefore, no illusion can arise from transferring the magnitude and shape of the one to the other. The illusions of sight are, therefore, perfectly consistent with our thesis, that there is a direct perception of extension (viz., that of the retinal pictures) by

means of that sense. But the illusions of touch are not consistent with the supposition, that we have an immediate perception of tactual extension and figure; because in this case, unlike that of sight, the extension and figure of the affected organ is identical with that of the external body.

This argument, with others adduced by Mr. Abbott and other writers, would, perhaps, justify me in accepting the conclusion of Platner, that our empirical knowledge of extension is derived from sight only, and that when a man *completely* blind from birth employs our space-terms he uses them with a totally different connotation (though with the same denotation) from that of the seeing man. The possibility of this may be thus proved. We will for a moment suppose the theory of Messrs. Mill and Bain to be fully established. Let us then imagine a man possessed of the sense of sight, but wholly destitute of that of touch and of the power of locomotion. We will further suppose some mechanical apparatus affixed to his eyes, and worked by a friend—without any consciousness on his part—in such a manner as to produce the succession of visual sensations which usually arise in the seeing man from the combination of muscular motions with the exercise of vision. Such a person would manifestly see visual differences between what others called circles, squares, cubes, right lines, up, down, far, near, &c., because it is admitted on all hands, that it is mainly the perceptions of these visual differences that suggest to us the tactual and muscular sensations which are supposed to be connoted by these terms; and it is further evident, that our Idomenian (as Reid calls him) would associate these visual distinctions with the terms which he usually heard applied to the objects in which they existed. He would, therefore, employ all our spacial terms, and employ them all correctly as regards denotation, though he could not have the slightest idea of the connotation attached to them by Mr. Bain. And here a similar concession must be made to Mr. Bain's blind man. He could certainly form

ideas, or collections of ideas, having the same denotation (or extension) as our space-ideas. But then the question arises, whether the Idomenian's idea of a square would be, at bottom, the same with that of the blind man? or, supposing them different, which of them agrees best with the idea of the ordinary man who can both see and move? To this last question Messrs. Mill and Bain reply that of the blind man, while Platner and Mr. Abbott reply that of the Idomenian. I agree with the latter; and I think Platner most worthy of credit, because he instituted careful observations with a view to decide the question, and was at first deceived by the appearances which must always be presented on the surface in such cases. It would be well to know whether the observations of Mr. Kinghan, (referred to by Dr. M'Cosh and Mr. Mill,) got beyond this first superficial stage in which the correct employment of space-terms is supposed to indicate the possession of space-ideas whose intension (or comprehension) corresponds with that of our own.

I need not discuss the question of the perception of space by other senses—including muscular effort—at greater length. I refer the reader to Mr. Abbott's work, Sight and Touch, for a fuller investigation of it. I will assume during the rest of my remarks, that these other senses either give us no direct knowledge of extension and magnitude, or else one so vague and rudimentary as to be incapable of correcting the space-perceptions of sight.* I will, there-

* I venture, however, to state, in a note, a few of my reasons for denying the perception of space to any other sense than that of vision. 1st. Let us observe that no conflict between sight and the other senses, in reference to the magnitude, unity, and position of objects, has ever been observed; nor has any reason been assigned why, in case of such a conflict, sight should give way to the other senses at all, much less at such an early period as to leave no trace of this conflict in the memory. We find sight very obstinate in some cases where it is evidently in the wrong, *e.g.*, the celestial motions and some cases of double vision. 2nd. The organs of feeling and locomotion, as already observed, are subject to much greater variations in different animals than those of vision.

fore, endeavour to deduce all the actual phenomena of spacial perception from sight only. The readiness with which these ideas seem *now* to be suggested by touch and locomotion is, in my opinion, the result of associations between them and certain visual impressions—associations which are very easily formed. If all the actual phenomena of space-perception are deducible from our visual sensations only, my theory is at least entitled to a fair hearing. It is quite as scientific as that of Messrs. Bain and Mill, and possesses certain advantages over theirs at the outset; while I shall endeavour to give, upon its basis, a much more complete explanation of the actual facts than they have attempted.

Starting, then, with the immediate perception of the pic-

Organs of feeling, or ascertaining *shape* by touch, many animals seem to have literally none. Yet, so far as we can judge, their space-perceptions are very accurate. 3rd. It is difficult to understand how we could ascertain the shape of any tolerably extensive object by touch and motion without sight; for in feeling it over (not to mention the difficulty of accurately retaining the series of feelings in their proper order in the memory) we do not feel some parts at all, and we feel others twice or even oftener, especially when deprived of the aid of the eye. But supposing all the feelings retained and put together in the proper order, it is evident that to attain a complete knowledge of the shape, we must have felt every part once, and no part twice. This seems to me to indicate strongly that our knowledge of tactual forms arises from visual associations; which is confirmed by the extraordinary difficulty experienced in ascertaining them by touch alone, when the form of the object handled is unusual and unfamiliar, and, therefore, but slightly associated with visual impressions. But sight takes in the shape at once, however unfamiliar; and, it may be added, that it reveals diversities of shape and figure too minute to be grasped by touch or sensible locomotion. 4th. Many men who judge of space with accuracy have not yet learned some of the equivalences which Messrs. Mill and Bain consider necessary for that purpose. A very simple experiment, suggested by Brown, suffices to prove this. Let us pass the hand over a book or desk, first rapidly and then slowly, with our eyes open. No difference in size is perceived. Perform the same operation with the eyes shut. The difference is startling, the object appearing enormously larger when

tures on the retina, and of these only, what progress can we make in explaining the actual facts of vision? The optician's principle that the character of the lenses &c. of the eye, as optical instruments, can be inferred from the actual phenomena, implies, as I have observed, that the change from the original to the actual percept is but a slight one. On the other hand, however, a formidable list of differences is made out even by those who admit that the retinal picture is originally perceived. Thus Dean Mansel says they differ in five important respects.* 1. The presented object is double, the represented (actually perceived at present) is single. 2. The presented object is inverted, the represented is erect. 3. The presented is very much smaller than the represented. 4. The represented object exists in three dimensions of space, while the presented exists only in two. 5. The presented

handled very slowly. We have not, in fact, learned (in the words of Mr. Bain) "to feel that a slow motion for a long time is the same as a quicker motion with less duration;" which he thinks we do by observing, "that they both produce the same effect in exhausting the full range of a limb." See also on this point Mr. Abbott's remarks, Sight and Touch, p. 34. In conclusion he observes, "A blind man will distinguish better by touch, because his touch is practised; therefore, clearly, ours is not. Is not this a demonstration that what we distinguish are not representations of tactual perceptions?" Lastly, Men have been born almost without the sense of touch and power of locomotion, and others have almost wholly lost these senses through paralysis; but I have never heard that the space-perceptions of these men appeared to be unusually deficient. Paralysis, indeed, seems to interfere with a man's power of localising his sensations when the cause which produces them is invisible, but it does not appear (except when the optic nerve is paralysed) to affect his perceptions of extension and figure in the slightest degree. I ought, perhaps, to have mentioned before that Berkeley himself remarks, that when we examine an object in the microscope we receive from it visual impressions, which are almost entirely free from associations with touch and locomotion; but extension and figure are, nevertheless, suggested to us as forcibly as when we view the same object with the naked eye.

* Mansel's Metaphysics, p. 80.

object is on the retina, and the represented is external to it. To this we might add that the presented object is bent in the concavity of the retina, which Sir William Hamilton urges as an argument to prove that the pictures on the retina are not the immediate objects of perception: for he says if they were so all objects must appear similarly bent.

1. To the first of these I reply, that the original percept or presented object is not always double, and that in many cases the present percept or represented object is double; In fact, I believe that whenever the one is double so is the other, and that, in this respect, education makes no change. It is evident that as the space which separates the two retinæ is not an object of visual perception, and, in fact, is not perceived at all, we cannot originally perceive these retinæ as separated by the space which really intervenes between them. It cannot, therefore, be determined *a priori* whether the two collections of pictures would be seen as wholly separate, wholly superposed, or partially overlapping; and in the first of these cases it does not appear to me that we could represent to ourselves the *unity* of space at all. But the bifurcation of the optic nerve at once suggests to us the probability that the two fields will partially overlap; for there is no reason to suppose that when two portions of the same nerve are drawn apart and separated by a piece of insensible matter, they lose their natural power of acting in concert, and suggesting the same, or at least immediately contiguous localities. The arguments that single and double vision, as actually experienced, depend upon original laws and not on education or association, are very strong, and the fact is by no means inconsistent with my theory. Nor is it easy to see how the unity of objects could be given us by touch or locomotion, while there are many cases in which, after all the information that these can give us has been obtained, we still see the objects double as before. I cannot, therefore, admit that there is in this instance any difference between the presented and the represented object.

2. To the second alleged difference a similar reply can be made. As long as it was held that touch and locomotion gave us accurate information as to the situation, &c. of external objects, the fact that no conflict arose between their presentations and those of sight, was supposed to establish decisively that we saw objects erect: but if, as I have contended, sight alone is directly percipient of the situation of objects, (or rather of the retinal pictures of objects,) and touch and locomotion only suggest it in consequence of previous associations with sight, there is absolutely no evidence for the universally assumed fact of erect vision. The whole problem, How can inverted pictures give rise to erect impressions of objects? seems thus analogous to King Charles's problem of Why a live fish does not add to the weight of the water in which it is placed, while a dead fish does? and the answer in both cases is the same, viz., that the assumed fact has not been proved. Indeed, even if touch could give us some idea of situation directly, it is not probable that it could do so with anything resembling the distinctness and minuteness of sight, and the conflict between the senses would naturally be decided in favor of that which spoke most clearly and authoritatively. But, in fact, touch does not seem to give us any such information. We are directly conscious of effort only, not *motion:* and that our voluntary efforts are accompanied or followed by motions we learn from sight, which, in presenting this motion to us, reverses its true direction. Vision, therefore, always continues to be inverse; a fact which is strongly borne out by some experiments of Scheiner with pinholes placed near the eye, which were successfully repeated by Porterfield and Reid.* Setting aside the line of visible direction, which I have already rejected, the only explanation that seems to me applicable to these phenomena is that our present vision is actually inverse.

3. Some of these remarks will also apply to the magnitude of objects. If the only space presented to us be on the

* See Hamilton's Reid, pp. 160-62.

retina, we can only compare the magnitude of the retinal pictures, and not that of the objects themselves. Hence no visible object can be larger than the whole retina, unless indeed we infer this by observing that the whole of it cannot be seen at once. When an object is distant, its image is small, and it looks small. When it approaches nearer, the image is larger, and it looks larger; and when it approaches to the nearest distance compatible with distinct vision, it looks largest, and it is largest. It is this last magnitude, I believe, that is usually meant when we speak of the real or absolute magnitude of objects; and if the object be brought nearer than this, it becomes so confused that its identity is no longer discernible. It can, however, be still seen distinctly, if we look at it through a pin-hole; and in this case, as was to be expected, the object looks larger than what is called its real magnitude. The visual image can then, under favourable circumstances, enlarge as well as diminish the real size of an external object. The mistake of supposing the presented object to be much smaller than the actually seen object, arises from the fact, that when we look at *another* retina, (or the reflection of our own,) the secondary pictures thus seen are very much smaller than the objects (pictures) which they represent; but in this case it is evident, that what is really seen is not the pictures on the retina looked at, but the pictures of those pictures imprinted on our own eyes by reflection from them. I can also prove by measurement, that the surface of the retina is confined to a square inch, whereas I can see yards and even miles; but what I in fact see are pictures of yards and miles on a very diminished scale; and when I measure another's retina and find it an inch square, what I really see is only the picture of that inch, on a very reduced scale also. If we distinguish between the presented and the represented retinæ—the pictures of external objects and the pictures of those pictures—we shall find no reason to believe that the pictures originally presented undergo any considerable

augmentation of size in consequence of experience and education. They remain in fact, I believe, of almost the same apparent magnitude as at first. Cases of couching for cataract strongly confirm this view. The patients never observed that the objects pointed out to them and named by their friends, appeared much smaller to the eye than they had appeared to touch and the muscular sense. There is, in fact, some evidence that, on the contrary, they appeared *larger*. (See Abbott, Sight and Touch, p. 145.) I am inclined to think, however, that by these expressions they only meant that objects appeared larger after couching than they had done *to the eye* before couching (for none of the patients had been totally blind.) Franz's patient, for example, on whom Mr. Abbott relies a good deal, thought he could see (previous to couching) "as much as one-third of a square inch of a bright object held within an inch or an inch and a-half of the eye, and in a certain direction." (Sight and Touch, p. 157.) This, of course, formed a limit to his distinct idea of the magnitude of a visible object; and when, after couching, he could see much more of the object, he naturally thought it larger than it had appeared hitherto. Those, however, who hold that we perceive distance, &c., by touch, may see in this fact an extension of the law that the least perceivable tactual interval appears greatest at those parts of the body where it is really least. Nor in these cases of couching does the object appear larger after comparison of our visual perceptions with those of touch than it did at first. Cheselden's case proves the contrary. "Being lately couched of his other eye," writes that physician, "he says that objects at first appeared large to this eye, but not so large as they did at first to the other." Their appearing *large* to the second-couched eye must, of course, mean *larger* than they did *then* to the other; and indeed the very words "not so large as they did *at first* to the other," can only be understood as implying a diminution of apparent magnitude during the interval that elapsed between the two operations. The

reader will further observe that the words "objects *at first* appeared large to this eye," seem to indicate that even in this second eye a diminution had set in at the time that Cheselden made his inquiries. I have dwelt on this point not merely in refutation of the Association Theory, but because I think Mr. Mill draws a wrong inference from Cheselden's remark, that "objects when seen with both eyes appeared about twice as large as when seen *with the first-couched eye only*." (Examination of Hamilton, p. 293.) Mr. Mill puts in place of the words I have italicised, "*with one eye only*," a substitution which the context proves to be erroneous, but on which his inference depends.

The perception of visual magnitude, however, requires a somewhat fuller explanation. The retina limits the perceivable magnitude of any object; and of objects larger than this we can only form a vague idea by observing that the whole of them cannot be all seen at once—an observation which could not be made until after considerable experience. But the real limit is, in fact, much less than this. There are always a number of confused pictures on the exterior parts of the retina, of which we are totally or almost totally unconscious. Thus many persons have never observed double vision, though double pictures are constantly in the field; and he who reads this page is scarcely conscious of seeing anything beyond it, though unquestionably many of the surrounding objects are painted on his retina at the same moment. What is really seen is, in fact, only the field of distinct vision, and the visible magnitude of any object will necessarily be judged of by the proportion which it bears to this field. But the extent of the field of distinct vision varies with the adjustment of the eye: and hence the actual magnitude of the picture of an object on the retina does not by itself determine its visible magnitude. For instance, let an object be placed at the nearest distance of distinct vision, and then slowly moved away. The continuity and similarity of the pictures seen lead us to regard them as a

single visible object, though, in fact, there are several different pictures, successive in time. Let us go on to inquire what change takes place in the magnitude of this object, which is thus judged to be one and the same? At first, as the image or picture diminishes, the field of distinct vision diminishes in almost exactly the same proportion. The magnitude of the object is, therefore, sensibly constant until it has moved for several yards—a fact fully borne out by experience, and which justifies my statement that the real magnitude of an object means the magnitude of its image or picture when placed at the nearest distance of distinct vision. Let us continue the motion. The field of distinct vision begins to contract more slowly than the distance of the object, and it now begins to appear of smaller size. But there is here another difference to be noticed. The picture becomes smaller *continuously*, while the size of the field of distinct vision alters *per saltum*, in consequence of the changes of adjustment and convergence which do not take place continuously. Hence the diminution in the seen size of the object (or its increase, if it be approaching from a distance) is not quite continuous. It sometimes appears almost constant for a time, and then undergoes an abrupt change. Experience again fully confirms this conclusion. Let me draw another. The confusion which leads us to pay no attention to the lateral parts of the retina, necessarily supposes a diversity of colours. If the whole retina was covered by one uniform colour, it would make no difference in the resulting perception whether the light were focussed on the retina or not; and, in like manner, when a considerable part of the retina is covered by a picture of uniform colour, the confusion is transferred to its boundaries, though these may be far outside what is, properly speaking, the field of distinct vision. A large uniformly-coloured object, therefore, practically increases the field of distinct vision, and consequently appears less than its proper magnitude. Wheatstone proved experimentally that the apparent magnitude of an object

is altered by a change of convergence, the magnitude of the eye-pictures remaining the same. The field of distinct vision diminishes with near convergence. The pupil also contracts in a strong light. This has often the effect of making a bright object appear larger.* Thus by this theory, and I think by this theory alone, can all the anomalous variations of visible magnitude be completely accounted for.

4. If Dean Mansel means that the presented object is *presented as* on the surface of the retina, I have already given reasons for holding that the local situation of these pictures relatively to the unseen parts of the face could not possibly be given in the original visual intuition: while if he means that it *is in fact* there, the same thing is true of what he calls the represented object. The real state of the case I take to be this:—The retina is originally presented to us, only in its character of a collection of visible figures and not in that of visual organ. The continuity and similarity of some of these figures convince us that, notwithstanding the changes they undergo, they represent (or rather *are*) the same objects. Some of these are either constantly in the field of vision, or can be made to appear in it at any instant by a voluntary effort. We find that changes in the space-relations between these and the other pictures are attended by sensations, and, above all, that we can produce (visible) motions in them by our own voluntary efforts. These are therefore in a peculiar sense *our* pictures, and they are, in fact, soon mistaken for our bodies. Part of the retina is therefore taken for the body, and as the eye is not pictured on itself, no part of it is taken for the eye. We soon learn from our perceptions of other men, and the reflection of our own form, to apprehend the space-relations which exist between our eyes and the visible members of our bodies; and when this stage is reached, we place (in imagination) an eye *behind* the collection of pictures which we have mistaken for

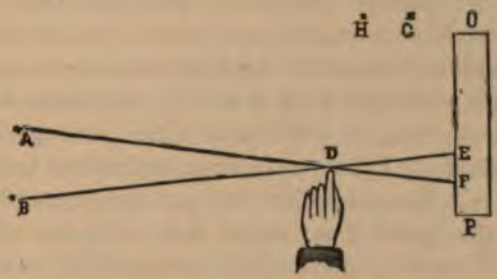
* On this whole question see Abbott, "Sight and Touch," pp. 57, 99, 100, 124.

our own bodies; and as we are conscious of no second set of pictures, we naturally enough conclude, when we learn the existence of the pictures on our retina, that these are not objects we were conscious of already. We judge our own eyes to have about the same visible magnitude as those of another man, (reflected on our retina,) or as seen by reflection in a mirror at the usual distance. This process, it will be seen, is the very reverse of that sometimes described. Experience does not teach us to project the pictures on the retina forward into external space, but to project the retina (if I may so speak) backward into an imaginary space situated behind the pictures which are really upon it.

5. The last difference noted by Dean Mansel is, that the presented object is extended in two dimensions, and the represented object in three. If this be meant literally, it is evidently not true. The retina, being a portion of a spherical surface, is necessarily extended in three spacial dimensions. But probably he means no more than that it is a regular surface with no hills and hollows in it—nothing corresponding to what we term relief. And here I think is, in fact, the greatest change that association and education effects. But then space, once presented in three dimensions, affords all the requisite elements for our perception of relief; and I believe that the education which vision undergoes in this respect is effected for it by itself.

In the first place, all that we see, or fancy we see, consists of surfaces in three dimensions, though no longer confined to a single spherical surface. We see surfaces at a distance from us, but it cannot I think be said in strictness that we see the distances themselves. Further, we see no surfaces from which rays do not come to the eye. No amount of experience will enable us to see round a corner; nor will the most perfect conviction of what would be seen behind an opaque object enable us to see it till the object is removed. Even in imagination we cannot represent ourselves as seeing more than the laws of vision permit, *e.g.*, as

seeing the entire surface of a closed solid of any kind. The difference here is evidently not so essential as to require the intervention of elements supplied by other senses, or by the *a priori* reason. But, secondly, sight affords everything necessary for its own education. I already remarked, that we observe, at a very early age, that contact between the pictures of our bodily members and those of other objects is constantly accompanied by tactual sensation. But this is not true of every contact. Apparent contacts are perpetually occurring which produce no such effect. Can we, then, find any criterion to distinguish *by sight* between contact attended by tactual sensation and contact not so attended? Certainly. Where contact is not attended by sensation it will almost always be found that we cannot at once see our limb and the object in apparent contact with it with perfect distinctness. When one is seen clearly, the other, if not double, is always more or less confused. The two eyes, in fact, do not refer the contact to the same precise locality. This is evident from the accompaning sketch. Let the two eyes be



placed at A and B, respectively, and let an object of any kind OP be partially obscured by the finger at D. Then to the eye at A the finger-point D will appear in contact with the point F, while to the eye at B it will appear in contact with the point E. Whenever this occurs the hand at D will experience no sensation of touch; while on the

other hand, if we can make the points E and F coincide, a tactual sensation immediately follows. Here then we have a visual distinction between contact attended with and contact unattended with tactual sensation, or as we afterwards call them, real and apparent contact. Now, is there any other mode of visually representing this distinction? Move the eyes to H, G and the separating lines DE and DF become visible. Visual contact, unaccompanied by tactual sensation, thus becomes associated with a visible distance which, though not seen at the moment, can be rendered visible by a change in the position of the eyes. E and F then come to be represented as farther off than D. The visual phenomena which distinguish apparent from real contact are, moreover, found to vary with the length of the lines DE and DF, becoming more marked as those lines are longer. In a word, these visual distinctions become marks of distance from the eye. The various pictures can now be no longer regarded as lying on the same spherical surface. We are obliged, if I may use the expression, to push some parts of this surface back and to pull other parts of it forward. Another circumstance which aids considerably in the education of vision is the disappearance of objects that are clearly within the field of vision. For example: I stand over a cup or jug, or a hollow in a field, seeing the interior very distinctly. I then move away, and almost instantly lose sight of it. But this object has not disappeared by getting beyond the limits of my visual field in the usual way, for I can still see the objects which surrounded it on every side. I am, therefore, compelled to regard it as still within my field of vision, though no longer visible, which I can only do by means of the representation of relief. A still more remarkable case is when I look at, perhaps, an extensive prospect over a wall or mound of nearly my own height, and then lose sight of it the moment I begin to move backwards. In all these cases it is easy to ascertain, by return-

ing, that the object is still there, and has not vanished like a dream. It often happens, too, that there is an inconsistency between the apparent distance of an object (judged of by confusion and duplicity) and the number and size of the intervening objects which are visible. This would, of course, occur where our position caused us to overlook a broad valley or chasm. But, perhaps, the most potent instrument in the improvement or education of vision is the different appearances which the same object presents when viewed from different directions. To take a familiar instance, a man appears quite differently when seen in front, from behind, and from one side. After a little time there can here be no doubt of the identity of the object seen; and by the ordinary laws of association, the visual appearance of the man from one point of view immediately suggests the visual impressions received from him when seen from other directions; but it is evident that these conflicting appearances cannot be attributed simultaneously to the same object, without supposing unseen spaces contiguous to those actually seen—a contiguity which would be impossible in most cases if these unseen spaces were outside the limits of the visual field—and it is these spaces (vividly depicted to imagination, with a full belief in their reality) which, in my opinion, constitute our perceptions of relief or solidity.

Why these perceptions are most vivid in binocular vision will be gathered from the diagram. The true distance of an object is, I apprehend, estimated by enlarging all the objects which intervene between it and some visible member of the body to their real magnitude, (*i.e.*, their sizes when at the nearest distance of distinct vision,) with a correction for objects overlooked, which is suggested by the presence of more confusion and duplicity in the object looked at, or in those close at hand, than the magnitude of the seen intervening objects will account for. Hence, if the distant object be uniform in colour so as to disguise these appearances, or if the intervening objects be cut off by a tube or otherwise, distance

from the eye is hardly suggested, and a distant cloud or piece of blue sky or the waters of a distant lake appear to be drawn across the end of our tube. This is the reason why (as noticed by Mr. Abbott) the presence of a varied lateral field is necessary to enable us to judge distance. The whole difference between our original and present visual perceptions can therefore, I think, be accounted for by associations between different visual impressions. It is evident that, as the law of Resemblance will here operate in addition to that of Contiguity, the association will naturally become closer than that assumed in the common theory; and as, on the other hand, I attribute much less to association than is commonly done, I think Mr. Bailey and Mr. Abbott can hardly accuse me to overrating the force of that principle. Of course I maintain further, that association connects tactual sensations and muscular efforts with the visual sensations which usually accompany them, and thus (after a time) enable these sensations and efforts to suggest to us the varieties of (visible) extension. But these associations are not, I believe, of a very close or powerful character.

Sir William Hamilton's objection with regard to the images being bent in the concavity of the retina is easily disposed of. If the fact that these pictures are bent rendered it impossible for us to see planes, his own theory that we originally see objects as planes would render it equally impossible for us to see concave surfaces. But that the bending of these pictures does, in fact, affect vision is proved by the unsuspicious authority of Mr. Abbott. "One or two circumstances," says he, (Sight and Touch, p. 82,) "deserve to be mentioned, which act as motives, so to speak, of the appearance of solidity. One is the spherical form of the retina, owing to which the images received on it cannot be projected or represented on a plane without distortion. One result of this is, that angles less than a right angle usually appear too large;" and the writer goes on to trace some

remarkable optical delusions to this source, for particulars of which I must refer the reader to the figures exhibited in his book. (p. 83.) They appear to me to constitute a decisive refutation of the plane-theory. As to Sir W. Hamilton's second objection, that if the retinal pictures were the immediate objects of vision we would require a second eye behind the first, I can only reply that I do not understand it. The light being focussed on the retina there is evidently no further need of lenses to concentrate it on the required points; and if the first retina cannot be an object of immediate perception the same remark would evidently apply to the second.

I do not consider the case of the lower animals, so often referred to, at all inconsistent with this theory. The distances which they know, (or at least prove that they know,) as has been already remarked, are not those of objects from the eye, but of objects from other members of the body which are painted on the retina; nor could the latter be deduced from the former without supposing a very complicated Natural Geometry. It is probable, I think, that in many of them the increase of confusion and duplicity with distance is more rapid than in mankind, and this gives rise to earlier and finer perceptions of the variations of near distances. Their superior powers of locomotion also afford more abundant and early experiences of the kind necessary to instruct them than is possible in the case of the human infant; and without at all contending that the lower animals are comparable to man in respect of intellect, it may be doubted whether the partridge of a week old is not a more teachable animal than the human infant of the same age. There is probably an implanted or instinctive desire in those birds that feed themselves to see the objects destined for their food with their greatest magnitude and distinctness. This wish directs them onward in a right line, (any deviation from this direction altering the appearance more or less,) and the moment confusion begins to result from its

close proximity, the object is within reach of the beak or talons. In fact, so far as I can judge, the mouths of all animals are placed at about the best distance from the eye for accurate vision, and also at the very verge of the visual field; so that passing the beak beyond the food which the animal attempts to seize is always followed by a confusion in its appearance and often by its total disappearance. How favourable to the instruction of the animal these circumstances are it is unnecessary to insist upon. I am convinced also that the cases of couching for cataract (which I cannot examine in detail) will be found quite in accordance with my theory. One peculiarity observed in many of them I may, perhaps, offer an explanation of—the fact that objects appeared to be in contact with the organ. To suppose that sight alone could reveal this fact is to suppose that the patient saw both the organ and the object, and made a mistake as to their relative positions, which I suppose no one will maintain. The truth, I apprehend, is this. The patients had previously judged of distance by *time*. A distant object was to them an object that could not be felt at once, but only after going through a series of successive tactual or muscular sensations. Hence they naturally expected that in the vision of the distant, the object would not be seen at the first instant, but that there would be a successive series of visual sensations terminating in a perception of the object. When this was found not to be the case—when the pictures which were said to be near objects, and those which were said to be distant objects, were perceived at once and in precisely the same manner—in what other way could the patient express this fact than by saying that they appeared to touch his eyes*? And as distant

* Another explanation however may also be offered, and one which is considerably favoured by some observations on couching. Prior to the operation the patients had not been totally blind. They had been able to see the colour, and somewhat of the shape of objects held close to the eye (*i.e.* to the cornea), and these perceptions had become more vivid and

objects appeared just like those which were near enough to come in contact with his cornea if he made any sudden motion, it is not surprising that he feared to move, and held up his hands before his eyes to guard them. But it was with his cornea, not his retina, that he apprehended a collision. In this point of view all that the observations prove is that distance from the eye is not an immediate perception of sight, which is an essential part of my theory of vision.

In conclusion, I shall endeavour briefly to reconcile my theory with the necessary properties of space and the elementary laws of geometry, which Messrs. Mill and Bain have, in my opinion, utterly failed to do. The necessary existence of space appears to me to result from the fact that space is always seen, even when our eyes are closed and all light excluded from the retina. In consequence of this, when we attempt to annihilate space we are attempting to annihilate an actual perception, which continues to exist during the attempt. Our belief in the infinity of space requires some further explanation. Cheselden's patient, who could not imagine space outside the room in which he sat,* could hardly have been at that time a firm believer in its infinity. But it is, nevertheless, probable that he could not imagine a limit to space; for the field of distinct vision, of which alone we are conscious, never possesses a precise and definite boundary. At a certain distance from the centre of the field, consciousness and uncon-

clear the nearer the objects were held. On being couched these patients saw forms and colours more distinctly than they had ever done before, and they naturally inferred that the visible objects were in very close proximity to the cornea—which is all they seem to have meant by saying that they appeared to touch the eye.

* This fact affords strong evidence, that even at this early stage space was to Cheselden's patient a *visible* object. His previous experience of touch and locomotion ought surely, according to Mr. Bain, to have precluded any such inability.

sciousness seem to melt into one another insensibly, and the mere effort to look at the limit of the field suffices of itself to make that limit shift farther off. Of course we may afterwards come to know that a picture which is near the centre of the retinal field is really farther from us than one nearer to the edge; but I am speaking of visual limits, and the ground at my feet is often nearer to the visual limit than the sun in the horizon.* The fact, then, that the field of distinct vision is always surrounded by other spaces, of which we have a kind of obscure semi-consciousness, renders it impossible for us to see or imagine a limit to space; and when to this we add our positive knowledge of its vastness as something surrounding and containing all that we see, (which experience soon teaches us,) these two elements seem to me to afford everything that is necessary to account for our belief in space infinite. As to its infinite divisibility, what I have already said as to the *minimum visibile* not being a quantity of space but a quantity of light, dependent moreover on the quality of this light and of that which surrounds it, as well as the condition of the visual organ itself, will explain a great part of the facts; and when

* This knowledge and representation of the greater distance of objects near the centre of the field than those near the edge is however of no use towards enabling us to imagine a limit to space; because it arises from the imagined appearance of the object, when seen from a different point of view in which we imagine ourselves seeing space beyond it. Indeed, these representations have the very reverse effect. When I look at what seems the most distant visible object—the blue sky, whose curvature is turned towards me—I am irresistibly led to imagine another point of view in which the curvature is turned away from me, and thus to suppose space beyond it. It should also be noted, that our only idea of a visual limit (of bodies or particular spaces) is a bounding line between *two* colours. Colour and space are thus supposed to exist at *both* sides of this limit; and this experience, therefore, renders it still more difficult for us to imagine a limit to *all* space. (See Hodgson, Space and Time, pp. 128, 139.)

to this we add, that very minute objects can be made by proper arrangement to exhibit a multiplicity of parts to vision, there is little left to account for. This little, I think, will be cleared up if we bear in mind, that in the *minima sensibilia* we do not drop at once from clear consciousness into total unconsciousness, but that objects pass from the one to the other by insensible degrees, just as daylight melts into darkness. The infinite divisibility of the intensity of sensation, or the possibility of reducing it by insensible degrees from clear consciousness to absolute zero, thus appears to me to carry with it the infinite divisibility of space.* The divisibility of time, perhaps, also assists us here; and still more assistance is afforded us by the fact of visual motion.

But why are the laws of geometry irreversible even by the imagination†? For similar reasons. The imagination

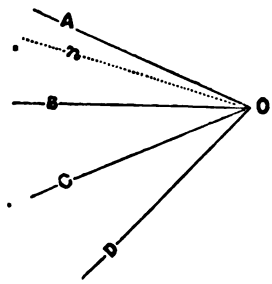
* It will appear from what has been said, p. 7, &c., of this Essay, that the visible line of demarcation which separates two coloured spaces is a breadthless one; and this divisibility by breadthless lines is, I think, closely connected with the phenomena of infinite divisibility.

† While philosophers are generally agreed in ascribing the most absolute necessity to the laws of Geometry, geometers usually admit that Euclid makes an assumption with regard to parallel lines, which is not self-evident, and of which no satisfactory proof has hitherto been given. The consideration of the infinity of space I think, however, really enables us to overcome the difficulty, and affords a proof which, if scarcely elementary enough for a schoolboy, may be found sufficiently satisfactory to those who have really felt the difficulty. It turns on the following principles: 1st. That if the legs of any angle be produced to infinity in one direction, the space that lies between them will bear a finite ratio to the whole plane infinitely extended in all directions; and 2ndly. That the space lying between a pair of parallel lines, produced to infinity in one direction, and terminated by a finite intersecting line at the other extremity, bears an infinitely small ratio to the same plane. (By parallel lines I mean lines which make internal angles together equal to two right angles with the particular intersecting line already referred to). Both principles can be proved by repeated superposition. For example, if A O B be

in representing figures and forms employs the organ of sight, and what cannot be presented on that organ cannot

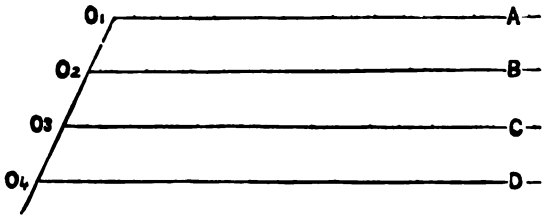
the angle in question, make B O C equal to it, and produce O C (fig. 1) to

Fig. 1.



infinity, and then the space lying in the angle B O C can be shown to be equal to that lying in the angle A O B by superposition. Repeating the same process for C O D, &c., we must, plainly, at length arrive at angle terminated by a line O*n* falling within our original angle. When this has been done, the sum of all the angular spaces will exceed the whole plane infinitely extended by the angular space A O*n*; and since they are all equal, if there be *n* of them, each will exceed $\frac{1}{n^{th}}$ of the whole infinite plane. To take the second case, (fig. 2,) let $O_1 O_2$ A B be the space re-

Fig. 2.

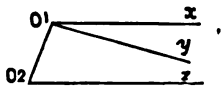


ferred to. Make $O_2O_3 = O_1O_2$, and draw O_3C, so as to make the angle $O_2O_3C = O_1O_2B$. Similarly draw O_4D, making $O_3O_4 = O_1O_2$, and the angle $O_3O_4D = O_1O_2B$. Then, by superposition, the spaces O_1O_2AB, O_2O_3BC, O_3O_4CD, &c., are all equal, and we can evidently cut off as many of them as we please without exhausting the whole plane. Each of them, therefore, is less than $\frac{1}{n^{th}}$ of the whole

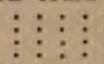
be represented by the imagination. The supposed visual appearances which we call two right lines enclosing a space, a plane triangle with one side greater than the other two &c., cannot take place on the retina, and therefore the imagination which uses that retina cannot represent them as taking place on it. When we endeavour to imagine two right lines enclosing a space, we (as Mr. Mill remarks) repeat the scientific experiment which proves the contrary. I must here deduce the subjective law of the imagination from the objective conditions of the nervous organism. But, on the other hand, I hold these laws to be necessary in the strictest sense; for it is not in consequence of early association that these combinations of lines and figures have become unrepresentable. It is the original constitution of our nervous organism that renders the requisite intuitions impossible; and so far as I can judge, the impossibility in question arises solely from the fact, that the organism is extended or exists in space. On this theory, therefore, we are able to maintain the objective no less than the subjective truth of geometrical laws, which on the *a priori* theory can only be done in connexion with Idealism.

plane infinitely extended. Now, to prove Euclid's famous principle let

Fig. 3.



the lines O_1y, O_2z make the angles yO_1O_2, O_1O_2z together less than two right angles. Draw O_1x so that the angles xO_1O_2, O_1O_2z shall be together equal to two right angles, and, if it be possible, let O_1y not intersect O_2z. Produce the lines O_1x, O_1y, O_2z to infinity, and if possible let the lines O_1y, O_2z not intersect. Then the space lying between O_1x and O_1y bears a finite ratio to the whole plane infinitely extended. It is therefore greater than the space xO_1O_2z, which bears an infinitely small ratio to the same plane; but it is also a part of it, which is absurd. Consequently the lines O_1y, O_2z intersect. Q.E.D.

The necessary laws of number are I think also consequences of the laws of visual intuitions *in space*. For example, the intuition necessary to prove that $a \times b = b \times a$ (an axiom which no mere time-intuition could prove) is of this description (for $a=4$ $b=5$);  where the number of the points is evidently the same, whether we add them up in rows or in columns.*

I conclude with a few remarks on the apparent magnitudes of the heavenly bodies. Everything that we see above us—clouds, birds, the tops of trees, houses, &c., appear largest when nearest to the zenith, for the simple reason that they are then nearest to us, and occupy the largest proportion of our visual field. Hence we come generally to consider object near the horizon (at least if above the level of the eye) as farther off than those near the zenith, and we alter their apparent size to correspond with their judged distance. Moreover the sun and moon seem fixed in the sky, and there is no deception in thinking that the horizontal sky is farther off than the zenithal. The blue sky is in fact determined in position by the points of mean diffusion of the blue sunlight which falls upon them, and these points are much farther off near the horizon, where perhaps there are four hundred miles of air dense enough to diffuse a sensible quantity of light than near the zenith, where there are not forty. This is the obvious reason why sky-intervals look larger for the same angular measurement,

* Arithmetic is a mere art, the theory of whose processes has generally to be derived from the science of Algebra: of which the rules for the extraction of square and cube roots afford a striking example. If the view here maintained be correct, the axioms of geometry and algebra could not possess *immediate* evidence for one who had always been totally blind, since he could not have the kind of intuition required to prove them. But he would, of course, believe in them as truths generalised from experience. A blind metaphysician might be able to throw some light on this question; but most men confound familiarity with immediate evidence to a degree that renders observation almost fruitless.

when near the horizon, and why the whole visible sky does not appear to be a hemisphere, but a much smaller segment of a sphere. In all this there is no deception, or if any, the deception is in the opposite direction from what is commonly supposed. The sun and moon appearing fixed in this sky are supposed to vary their distances from us as the sky does.

I need not here discuss the (so-called) association between colour and extension, which has puzzled so many inquirers. I do not regard it as an instance of association at all. The colour and the extension which I see are both in the same place, namely, on the surface of the retina. I cannot see colour without extension, because colour is only presented to me in an intuition of this extended retinal surface. I cannot see extension without colour, because extension is only presented me in an intuition of the coloured surface of the retina. I cannot imagine either of them without the other, because imagination is, equally with perception, a veritable affection of the retina in which colour is presented to me as extended, and extension is presented to me as coloured. Here again there is no delusion. On the contrary, the inseparability of the two perceptions becomes obvious as soon as we recognise the retinal images as the only direct objects of sight.

This theory, then, appears to explain all the ordinary facts in a much more complete manner than the common or Berkeleian one. It is free from several objections which can be urged against the former, and it is supported by some arguments which seem applicable to it alone. On all these grounds it appears to me entitled to a fair hearing, and must, I think, be refuted before either the Berkeleian, the *a priorist*, or the disciple of Mr. Abbott can submit his to the public as exclusively capable of accounting for all the phenomena. It is very far from being perfect, and very far from being demonstrated—as, indeed, every new theory when first submitted to the public must be—but, on the

other hand, it seems to me quite sufficiently advanced to be considered as a scientific hypothesis equally admissible with that usually accepted, and equally deserving of investigation by scientific men. An analogy from the corresponding branch of physical science may perhaps illustrate its possible importance. The emission theory of Light, like the Berkeleian theory of Vision, reigned supreme for almost a century after Newton proposed it. It explained most of the known facts, and led to the discovery of others. Yet, after all this, another theory was started—at first cautiously and without full proof—in opposition to it. The new theory, though supported by less famous names, soon made progress. First, its explanation of the facts was found to be more complete than the Newtonian; then it led to the discovery of new facts, and the explanation of others discovered by chance; and finally, *experimenta crucis* were discovered by scientific inquirers, and the result was the complete overthrow of the once-famous optical theory of Newton, and the installation of the Wave-theory of Light—now considered one of the most beautiful and far-reaching discoveries in the physical sciences—in room of the dethroned monarch. Whether a similar fate awaits the theory advocated in these pages time alone can determine.

SUPPLEMENTARY NOTES.

1. *On the Association Psychology.*

As their theory of Space and Vision is perhaps the greatest stronghold of the Association School, it may not be out of place to add a few general criticisms on a philosophical system which is certainly the most pretentious, if not the most popular, of those now current. These observations could not have been introduced into the text without interrupting the thread of the argument.

The Association Theory as at first advocated, was, I think, a consistent and intelligible one. It pre-supposed nothing in the human mind except a capacity for receiving sensations, and a power of associating these sensations; from which it undertook to explain all the mental phenomena. Its explanations however were, I think, frequently defective. It despised what Mr. Mill terms Introspection, which, if not the chief mode of advancing Psychology, is at all events an indispensable preliminary step. It is absolutely necessary to ascertain, by careful introspection, the actual characteristics of the phenomenon we are going to explain, or else instead of accounting for the facts we shall account for a part of them, or perhaps for some fiction of our own. This is the great fault of Hobbes' account of our active principles, as Bishop Butler pointed out long ago; and Hobbes may be almost regarded as the founder of the Association School. It is also, according to Mr. Mill, the chief defect in Brown's Theory of Space; and that Mr. Mill's own theory is not exempt from it will, I hope, appear from the preceding pages. In particular, I cannot find either in his theory, or in that of Mr. Bain, any explanation of why all spaces are regarded as parts of one vast space, or why this one vast space has three dimensions and no more. There was also another difficulty always attached to the Association Theory, namely, How are we to determine what elements are to be assumed as (at least provisionally) ultimate in order to explain the

the others? Suppose, for example, that *a*, *b*, *c* are three ultimate elements, and that associations among them produce a fourth element, *d*. In such circumstances it is pretty evident that an ingenious theorist who started with the assumption that *a*, *b*, and *d* were ultimate elements would have little difficulty in giving a very plausible explanation of *c*; especially if he did not investigate the actual characteristics of *c* too minutely. This, if I mistake not, has been frequently done. Those who explain space by means of motion afford an extreme instance of it, and it is still a matter of dispute whether space is to be explained by simultaneity or *vice versâ*.* Again, a distinction between the "mass or volume" of two sensations has been introduced for the same purpose. But it seems to me very probable that this distinction is dependent on our space-perceptions instead of the contrary. I doubt if a blind man could say whether the sensation arising from putting his foot into hot water up to the ankle was more or less voluminous, (if he could attach any meaning to this phrase,) than that which arose from submerging his arm to the elbow. My knowledge of spacial magnitude enables me to ascertain that the immersed portion of my body is greater or smaller in the one case than in the other, and I judge of the mass or volume of the sensation accordingly.

But ordinary Association was found insufficient to accomplish the task set before it, and a new kind, designated Chemical Association, was called in to supply the deficiency. It was now assumed that a collection of ideas intimately associated with each other ultimately became fused together, and gave rise to a product, in which the separate elements could no longer be discerned. The analogy to chemistry, however, was evidently incomplete. The chemical elements are permanently-existing substances which continue to exist in the compound, and into which the compound can again be resolved; but our sensationsan d ideas are not permanent entities, but fleeting existences, whose *esse* is *percipi*, and which cease to exist when they cease to be perceived. The resultant idea could not, therefore, be regarded as composed of the simple ideas in the same sense, that the compound body is made up of the simple

* Mr. Mill's arguments on this latter question would not appear so formidable, if he had distingnished between a complex sensation whose elements are known, and two or more distinct simultaneous sensations.

chemical elements. This difference is manifest in one most important particular. The (supposed) compound cannot be again resolved into its (supposed) elements; while this is precisely the characteristic that distinguishes chemical composition from ordinary causation. The chemical compound, too, may be formed the first time that the elements are brought together, as easily as on any subsequent occasion, but the resultant idea is not supposed to arise until repeated juxta-position has produced an intimate association among its elements. The asserted analogy is therefore confined to one or two subordinate features, and fails in what is most characteristic of the chemical phenomena. This may be considered an objection rather to an illustration of the theory than to the theory itself; but the truth is that what enables us to prove the existence of chemical composition are precisely the features that are wanting in this alleged mental chemistry. We believe that water is composed of oxygen and hydrogen, because we can mix these gases and convert them into water at once by the passage of an electric spark, and because we can again convert water into oxygen and hydrogen by sending an electric current through it. If there was no possibility of performing either experiment we should never have suspected that water was a compound, still less could we have ascertained that it is made up of oxygen and hydrogen. Now this is the position in which the Associationist finds himself. He does not know what ideas are elementary, and even if he has reason to suspect some of them to be compound, he has no means of ascertaining their composition. In this exigency he resorts to conjecture and hypothesis. I am not certain how far Mr. Mill and Mr. Bain regard the idea of space as analogous to a chemical compound,* (rather than a mere mechanical juxta-position of the elements;) but I may be permitted to make that supposition for the sake of illustration. How is space shown to be a compound? For if it is admitted that the elements are not separately perceptible in it, this cannot be discovered by introspection. If it is a compound, how are we to ascertain the elements? Only by hypothesis, so far as I can see. Again, how are these elements proved to be simple? for the chemical theory supposes that an idea may appear simple and yet be really compound. Lastly, how is it proved that space does not form

* I think, however, that Mr. Mill has said so in some of his works.

an element in their composition? for on this theory it might form an element in them, without being detected, (not to mention that the opponents of the theory profess to have actually detected it.) In this particular case, too, the elements are not proved to have been in the mind before the idea of space, which latter idea seems to be in the mind of the infant as soon as it is accessible to observation. Nor are the supposed elements proved to have been in the infant's mind, and to have been subsequently lost. On the contrary, all the sensation-elements continue to be found in the adult man. The elements, then, do not disappear into the resultant as in chemistry. The whole theory seems to have been invented, in order to make out an explanation of all the phenomena of mind by the single principle of Association. But in reality, even granting its truth, it fails to accomplish this purpose; for this (so-called) Chemical Association is manifestly a totally distinct principle from ordinary Association, and ought, therefore, to have a distinct name of its own. There is no extension of the principle, but only an extension of the name. And, indeed, this remark will be found to apply to a large proportion of these professed discoveries. When, for example, we are told that a concept or a judgment has been proved to be a "case of association," it will usually be found that these and the like operations have merely been analysed in two or more elements and a relation or connexion found between them; which every one knew before. And in the same category I should be disposed to place the great modern discovery of Relativity. No one who has read Bacon ought to imagine that Psychology can be converted into a science by means of this kind of *axiomata generalissima*.

I, therefore, hold the Chemical Association theory to be unproved, and that the proof of it is surrounded by difficulties, which its advocates have in a great measure failed, not merely to overcome, but even to apprehend. But I do not mean to reject it as either refuted or as unprovable.* There is one way in which I think it might

* Mr. Mill would, I suppose, contend that this Chemical Association was proved by the instances produced by him, in which the elements of cognitions disappear in the wholes, provided the latter alone interest us. (Exam. of Hamilton, p. 315.) His chief example is the act of reading a book in which we begin with the letters and syllables, which almost entirely drop out of subsequent consciousness in the representation of the "general purport of the discourse;" just as we lose the individual leaves in retiring from a forest. But

be proved. We might be able to detect the elements first in mere juxta-position, then slightly associated, then more and more closely connected, and at last, in a kind of incipient fusion, which left no doubt that the resultant idea was the final stage of the process we had been tracing. It might also attain a high degree of probability, if it were ascertained that there were certain sensations associated with each other in the child's consciousness before the idea we sought to explain was found there, and that these sensation-groups had disappeared as soon as the (supposed) resultant was ascertained to exist. But I do not think that this has hitherto been done; and the process, if proved to exist, would rather resemble the growth of a plant or animal than chemical composition. And moreover, it might be found that the development of the ideas depended on laws quite distinct from those of Association.*

But the old Association Theory was only a consistent theory of the mental phenomena, on the condition that the sensations with which it started needed no Psychological explanation. Assuming these, it derived the laws of belief and imagination from them at once. We believed this because we had always experienced it to be so: we could not conceive that because we had no experience of it. But here the Associationist really takes for granted more than the mere sensations. In fact, he assumes what Mr. Mill does in his explanation of our belief in the existence of an external world, namely, "1st, sensations; 2nd, succession and simultaneousness of sensations; and 3rdly, a uniform order in their succession and

this example seems to me to involve two errors. For, first, we do not begin with the individual letters, but on the contrary, we limit our attention to them by a special effort; indeed, we have seen Mr. Mill himself adopting Mr. Bain's statement that "our notions of form are manifestly obtained by working on the large scale." (Id. p. 291.) And secondly, if the "general purport of the discourse" is a whole, (rather than a selection of the more important parts,) it is certainly not the whole, *of which the written characters are parts;* and my idea of *this* whole does not seem to be clearer than my ideas of the individual characters. Nor can I see any force in his analogous case of sensations. (Id. p. 308.) For in this case of the revolving cylinder, though the causes of the various sensations act in rapid succession, the sensations themselves (in consequence of the well-known persistence of visual impressions) are rigorously co-existent; and therefore the effect is the same as that which would be produced by the co-existence of their causes.

* See on this theory, Mansel's Metaphysics, p. 247.

simultaneousness." (Exam. of Hamilton, p. 249.) Here a doubt arises. An uniformity of succession or co-existence among sensations is in other words a law of sensations. Ought you not, then, to assign a place to this law in your Psychological system? To this the older Associationist replied—No. It is not a law of mind at all. There is a real world existing without me, which acts on me, and produces these sensations; and the uniformities of succession and co-existence which they exhibit are the results of the laws of this external world, They are, therefore, out of the province of Psychology. In the mind there are but two things, a capacity for receiving sensations, and a power of associating them. What particular sensations will be received, and what uniformities these sensations will exhibit, depend on something quite distinct from the mind; and the necessities of thought, which our opponents represent as mental laws, are produced by these laws of the external world through the medium of Association. From the realistic point of view, this answer was quite satisfactory, and moreover, it seemed to to afford a mode of escaping from the idealistic conclusions of Kant. That philosopher had inferred from the necessity of geometrical judgments that they depended on laws of mind, and that therefore space, and everything which it contained, existed in the mind only. But the disciple of Hartley could answer—The necessity of the judgment only proves the uniformity of the experience on which it rests, and this uniformity of experience is the surest indication of a law of the outer universe. Geometry, therefore, is universally true of objects independent of the mind, and space is a property of these objects.

But when Associationists began to investigate how Association operating on mere sensations might give rise to the belief in a real world independent of us, the conditions of the whole problem were altered. The existence of such a world could no longer be assumed. Indeed the explanation showed that we should equally believe in it if it did not exist. But the theorist was still obliged to assume uniformities of succession and co-existence among his sensations; and as these sensations existed nowhere but in the mind, and did not depend on any non-mental causes, their uniformities or laws plainly became the subject-matter of Psychology. Nothing exists but actual and possible sensations, and their existence consists

in their being actually or possibly perceived. They and their laws belong exclusively to mind. This consequence the Idealistic Associationists seem to me to have entirely lost sight of. They laboriously seek to generate from Association operating on these uniformities the very laws which form the scientific expression of the generating uniformities themselves. Thus, for example, we meet with a long discussion, endeavouring to deduce the necessity of geometrical laws from Association. I cannot conceive two right lines enclosing a space. Why? Because I have always experienced them diverging, and hence the idea of divergence becomes inseparably associated with that of two intersecting right lines; and, in consequence, I cannot now conceive them as approaching again. But why is my experience uniform? or rather, what does this uniformity imply? Simply this, that my mind is so constituted that I cannot (or, at all events, do not) perceive two right lines enclosing a space; and this being granted as a law of sense, it is much more philosophical to extend this law to explain the similar phenomena of imagination, than to suppose the latter governed by a different law, generated by a slow process of Association operating on the former. When the motions of the earth round the sun were traced to the combination of a centripetal and centrifugal force, it was surely more philosophical to trace the similar motions of the moon round the earth to the same sources, than to suppose that the latter did not gravitate, but had a tendency to imitate the motions of surrounding bodies, which, as its imitative faculty grew stronger, gradually became more accurate, until at length it came to pursue the precise path which it would follow if subject to the law of gravitation. In this particular case, moreover, Mr. Mill sets up an explanation which I think inconsistent with the Association Theory; namely, "the exact resemblance of our ideas of form to the sensations which suggest them." (Logic, vol. i. p. 262.) If this explanation be correct, we could never have imagined two right lines enclosing a space, and the impossibility was not, therefore, a gradually-acquired one. But here again the resemblance is assumed as a fact, without referring it to its proper place in a system of Psychology. Kant accounts for it. Space, says he, is a law of our faculty of intuition, and imagination being a branch of this faculty, is, of course, subject to the same laws. Hence geo-

metrical truths are equally binding in perception and imagination, and in both cases for the same reason. Is not this, at all events, simpler than assuming separate uniformities of co-existence among our sensations for each of the fundamental laws of geometry; and then either a long associating process to generate similar uniformities of co-existence among the corresponding imaginations, or else a new law of resemblance between them limited (at least in this degree) to this particular instance? It is the office of Philosophy not to multiply laws, but to reduce them to the smallest number. Imagination has often been described, even by Associationists, as a kind of weak sensation or decaying sense, while it is now believed that its representations take place in or by means of the sensitive organ. What is more natural, then, than to suppose that the laws of our sensations embrace the products of imagination also? But if the reader wishes to see a complete theory of the subject, he should take up Kant's Transcendental Analytic, where he will find the laws of sensations assumed by Mr. Mill reduced to a few general heads, and connected with the laws of thought recognised by logicians in a manner that, in point of systematic connection, completeness, and simplicity, is as far superior to the Association Psychology as the Copernican Astronomy is to the Ptolemaic. I do not mean, of course, to affirm the truth of Kant's conclusions. Those of the Associationist may ultimately prove more nearly correct. But the Association school boasts loudly of the scientific character of its method and its results, and these are exactly the points in which it seems to me most glaringly deficient. It may, perhaps, have got hold of a sound theory. But its nomenclature is faulty; it fails to see the conditions of proof, assumes what ought to be explained, leaves the consequences half thought out, and is wholly wanting in system. It professes to adopt the method of Physics, but presents us with speculations that no natural philosopher would tolerate, even if he did find it necessary to cut the theorist short with some such remark as—"You undertook to give me a theory of dew, and you are accounting for rain."

Mr. Mill thinks that (with perhaps the exception of the principle of contradiction) all our present mental necessities or inconceivabilities would cease, "if our minds were the same, but our experience different." (Exam. of Hamilton, p. 84.) What does this

mean? Have our "minds" nothing to do with our "experience?" Is not our experience, on his own showing, merely so many actual or possible mental sensations, whose laws are laws of mind? He gives us, however, some specimens of the experiences that would be required to enable us to conceive the reverse of truths commonly regarded as necessary. Suppose that a *minimum visibile* was not increased in magnitude, when we used a powerful microscope to examine the object, we should be able to conceive a limit to the divisibility of matter. (Id., p. 102.) The minimum visibile is, I think, a quantity of light not of extension, so that Mr. Mill starts with an erroneous assumption. But waiving this objection, let us examine the case he puts. He takes an object containing 100 minima visibilia, and puts it into a microscope magnifying fifty times, and then he tells us that if each minimum visibile looked the same size as before, and if the whole object made up of the 100 minima visibilia looked fifty times larger, we should believe in a limit to the divisibility of matter. Certainly; and if the same minimum visibile was presented to me as all black and all white at the same instant, (see Exam. of Hamilton, p. 84,) I could go a step further, and disbelieve the principle of contradiction (for I fear I must always regard black as not-white.) But it seems to me that so long as "my mind is the same," both these "experiences" are simply impossible. My experience, I believe, is in part determined by my mind; and Mr. Mill, on his idealistic principles, is bound to maintain that it is so entirely. The barrister, I think, makes a similar blunder when endeavouring to prove that the reverse of arithmetical truths might be made conceivable by a different experience. He supposes that whenever I attempt to put two and two together, a new object is "created and brought within the contemplation of my mind," (Id., p. 86,) and argues that in this case I would believe that 2 and 2 make 5. But while my mind is the same, I cannot be compelled to *add* the new object, thus brought within its contemplation, to the 2 and 2 which I am seeking to put together; and even if my mind was so altered as to make this addition compulsory, I could not fail to see that what I had added was not $2 + 2$, but $2 + 2 +$ *the new-created unit;* so that I would have really arrived at the harmless result, $2 + 2 + 1 = 5$. Will Mr. Mill or his barrister maintain that in a world, where the two pairs were anni-

hilated whenever I attempted to add them, $2 + 2$ would be $= 0$? Some of Mr. Mill's other instances are mere tricks with words. If I hold a circle sideways it appears to be an ellipse, and if a man had never felt or seen it in any other position, he would use the term "circle" with the same connotation as my term "ellipse," and he would, of course, believe that the radii of a circle are unequal. Translated into my language, this would amount to conceiving that the radii of an ellipse are unequal, which I can conceive with equal ease, though my experience has been different from his; and as he would not have *my* idea of a circle at all, he could not conceive anything about the equality or inequality of its radii. This is exactly the trick that the barrister performs with his right lines, (Id., p. 86,) and that Reid enlarges on in his Geometry of Visibles (cited, p. 87.) If right lines looked like circles, we should ascribe to them the properties of circles, at least if we had no means of correcting the mistake. Accordingly, to Reid's Idomenian, it would be a "self-evident truth," that "any two right lines, being produced, will meet in two points;" but the Idomenian would mean by this proposition exactly the same thing that I mean by the proposition, that "any two great circles described on a spherical surface will meet in two points;" which is to me a self-evident truth likewise. Reid never supposes that the Idomenian would ascribe any property to his right lines that I do not ascribe to circles on the sphere, and thereby he recognises the necessity of geometrical truths, if we only take care that the same term shall have the same meaning. Had Mr. Mill forgotten his own doctrine that the meaning of a word resides in the connotation, not in the denotation? I fear Helmholtz has here fallen into the same mistake.

I may further observe, that I am not yet satisfied that Association can generate necessary beliefs. Mr. Mill's examples (Exam. of Hamilton, chap. xiv.) are not examples of necessary beliefs or judgments at all, and it is only in judgments that necessity is generally insisted on as establishing a mental law. They are, moreover, in general instances of a "could not," not of a "must," and it is, I think, conceded that a present inability need not be *a priori*. For instance, whenever there is a series of acts to be performed, which requires memory, anything that confuses the memory may produce

an inability to perform it; and as memory is admitted, on all hands, to depend chiefly on association, a change in the usual associations will naturally produce an inability here.* There is something similar to this in positive necessity also. One idea will recall another which is associated with it, in spite of all our efforts to resist it. In fact, forgetfulness is even less in the power of the will than memory. But though the suggestion is involuntary, I am not compelled to predicate one of these ideas of another. I may be unable to think of Mr. Mill without thinking of Mr. Bain, but that does not compel to judge that Mr. Mill *is* Mr. Bain, or even that the two philosophers are inseparable in existence. What the Associationist is called on to explain is the characteristic of necessity *in judgments*, and this is exactly the kind of necessity which he avoids giving instances of. It may be that in every familiar judgment the subject and predicate are associated; but the problem which Mr. Mill and his followers have to solve is not why they become associated, but why the association between them takes this particular shape. There are pairs of phenomena, between which the association is most intimate, but which we never dream of combining as the subject and predicate of a proposition. Consequently, if this be a "case of association," it is, at all events, a peculiar case, and it is just this peculiarity which requires to be accounted for.

In conclusion, I may remark, that the earlier Berkeleians maintained that our immediate visual sensations were at once replaced by the suggested tactual ones, and were themselves either unperceived or instantly forgotten; and they were quite satisfied with the explanation of this substitution, which the principle of Association afforded. The later advocates of the theory, as we have seen, admit that the present representation is wholly an "eye-picture;" and they call in the same potent principle to explain why

* I may remark that Mr. Mill is in error in regarding necessity and inconceivability as correlatives, for under the law of the conditioned, two contradictories may be both inconceivable. The necessity of so thinking, of course, involves the inconceivableness of the contrary, but the inconceivableness of a judgment does not involve the necessity of the opposite. This confusion leads Mr. Mill into several unfounded criticisms on Hamilton, in whose system the distinction of positive and negative necessity (inconceivability) is all-important. (See Lect., vol. ii. p. 366, seqq. and 527.)

the suggested tactual sensations either do not rise into consciousness at all, or else leave no trace in the memory. When we find the same principle invoked with equal confidence to explain two directly opposite (alleged) phenomena, the natural presumption is that it really explains neither; while, on the theory of Messrs. Mill and Spencer, the Natural Geometry, which Berkeley treated so contemptuously, has to be replaced by a still more complicated Natural Algebra. Such explanations are, to say the least, eminently unscientific.

2. *On Colour-Blindness.*

Some persons may perhaps consider the case of the colour-blind a strong argument against the theory that the perception of a variety of colours involves that of extension; since it may be said, here are men who can see no variety in colours, and whose space perceptions nevertheless appear quite as accurate as those of persons who do. But all observations on colour-blindness lead to the conclusion that the patients do perceive a diversity of colours (and this not mere gradations of the clare-obscure, as Sir W. Hamilton supposes) though they are unable to discriminate between some particular colours which ordinary men distinguish with ease. Thus the man who confounds red with green will often distinguish yellow and blue with much accuracy. It is not my intention here to make any hypothesis as to the nature of colour-blindness, (which, however, I have done in the numbers of *Nature* for July 28, and October 6, 1870,) but merely to notice that we here possess what Mr. Mill wishes to obtain in the case of the blind—a colour-blind metaphysician. Dugald Stewart could see no difference in colour between the green leaves and the scarlet fruit of the Siberian crab, though he could perceive the difference of shape perfectly. (Nunnely, on Organs of Vision, p. 336.) Yet Stewart stated in one of his earliest letters, and repeated it in his latest work, that he could not subscribe to the doctrine of Reid on this subject, "because it appeared to him evident that the varieties in our perception of colour are the means of our perception of visible figure." (Dissertation, note, p. 134.) It is strange that with this letter before him, Sir W. Hamilton should have in his Lectures credited Stewart with the doctrine that we have no perception of extension by sight,

(Lectures, vol. ii. p. 161,) a statement which he qualifies in his edition of Reid, (p. 920,) by saying that it was tacitly withdrawn in his later works; whereas, in the note already referred to, Stewart is professedly quoting a letter which he had written to Reid "more than forty years before." It is true, indeed, that Stewart was of opinion that the inseparability of colour and extension in the imagination was a mere result of association; but then this association arose from the fact, that the varieties in our perceptions of colour were the means of our perception of visible figure. If he had been of opinion that the imagination employed the sensible organ, he would probably have dispensed with association altogether. But at all events it is evident that he thought the perception of a variety of colours necessarily led to the perception of extension, and that he found the two ideas of colour and extension as inseparable as anyone else. I think his testimony on these two points affords a strong confirmation of the theory which I have been advocating.

3. *Observations on Couching.*

I venture here to make a few remarks on Observations on Couching, because I think they have not hitherto been made with sufficient care. For example, when the newly acquired sense is tested with a cube or a pyramid, it is forgotten that such an object will look differently when it is seen from different points of view, and that the patient might very possibly fail to recognize its identity if successively presented to sight under a diversity of circumstances. It would be much better to employ discs than solid bodies, though even here the visual appearance would vary with the inclination of its sides (if a rectilineal figure) to the line of sight. A circle and a square should therefore be substituted for the sphere and the cube, and it should be ascertained that the patient recognised the identity of the square when presented to him at various angles. We should next endeavour to make him generalize these perceptions, and abstract from the particular colour and magnitude of the presented objects, for which purpose sveral circular and square discs of different sizes and colours should be successively (or simultaneously) presented to him. It would also be desirable to quicken his faculty of generalization by giving them general names; and in so doing, names should be selected which were quite free from all previous associations. Suppose, for example, (as the patient would not be

likely to know Greek,) the square discs were called Alphas, and the circular discs Betas. Care having been likewise taken to secure the formation of the requisite touch-abstractions, (by handling several circular and square discs of various sizes, weights, and degrees of hardness, roughness, &c. without seeing any of them,) he might then be asked whether an Alpha or a Beta was like anything he had been accustomed to feel? and if he failed to recognize the identity, he might be asked if either of them was like a circle or a square? If after several trials the patient was found incapable of recognizing the identity of the objects seen and the objects felt, it would prove that the perceptions of the two senses were dissimilar in kind; but it would still remain undecided which sense-perception was identical with that which ordinary men designated space or figure. If the patient was asked some months afterwards whether his ideas of circles and squares had been altered since he obtained his sight, or whether the meaning that he now attached to these terms was a visual or a tactual one, it would throw some light on this subject. The meaning which he attached to these terms immediately after couching, would of course be the same that he had attached to them before couching, viz., a tactual one; consequently no inference could be drawn from his at first applying these terms to tactual, and denying them of the visual objects, except that the two senses did not convey the same kind of perceptions. It is too often forgotten that different men may employ the same terms with the same denotation, but with a totally different connotation;* and this at all events *may* be the case with the space-

* Nothing, perhaps, has led to more error and confusion in Psychology than this oversight. Instances of its ill effects have, I think, been pointed out in the preceding pages. Thus, if Mr. Mill *means* by space something which the hand could be moved across, and if by moving the hand he *means* a succession of muscular (not visual) sensations, to say that sight cannot give us a direct perception of space is an identical proposition, and it is a mere waste of time to spend more than three lines in proving it. But then I do not think that these are the meanings which ordinary men attach to the terms Space and Movement. To take another instance. We often meet with an argument to prove that some proposition—the principle of Causality suppose—is not a necessary truth, because some one has disbelieved it, when it will be found on examination that the believer and the disbeliever merely attach different meanings to the same form of words. This, as we have seen, is the explanation of the fact (if it be one) that Reid's Idomenian would find no difficulty in believing that two right lines enclosed a finite space. Again;

terms of the blind man and the seeing man. What I mean by "circle" and "square" might very well be what the newly-couched man would mean by "Alpha" and "Beta" in the case I have put, though he would also use the words "circle" and "square," and apply them to the same objects that I did. I should also prefer a triangle and a square to a circle and a square. The uniformity of the circular outline would give considerable scope for guessing if the patient was at all quick. The whole subject requires much more attention than it has hitherto received.

in the controversy as to whether children commence with universals or singulars, philosophers have confined themselves almost entirely to inquiring whether they begin by using terms which are *to us* universal or singular; which probably depends altogether on their instructors, and not on any peculiarity of the infant mind. But perhaps the most remarkable example of this confusion occurs in Mr. Lewes's criticism of the Kantian distinction between analytic and synthetic judgments. This writer concludes that there is no real difference between them, (synthetical judgments being only "analytical judgments in the making,") because the same proposition—that is, the same form of words—that originally expressed a synthetical judgment (sometimes) comes afterwards to express an analytical one, *in consequence of a change in the meaning of one of the terms employed.* Thus, that "All air is heavy" was originally (and I think still continues to be) a synthetical proposition; but in the course of time the word Air may come to include Weight in its meaning, (connotation,) and whenever it does, the above proposition will express an analytical judgment. This analytical judgment translated into my former language would be "All heavy-air is heavy," and my former assertion that "All air is heavy," translated into my new dialect, would be "Whatever possesses the *other* attributes of air possesses the attribute of weight also." At the basis of the whole theory, therefore, there lies nothing more than that men may attach different meanings to the same form of words at different times. When Weight becomes a part of the meaning of the term Air, of course an opponent could not deny that the air was heavy; but Mr. Lewes seems to have forgotten that he might still deny that any such thing as air existed. He might very well argue that the thing that blew in his face was not Air, since it was not heavy; and if Mr. Lewes wished to convince him of his error, he could only do so by proving that that which blew in his face was heavy—a synthetical proposition. Mr. Mill has done good service in refuting similar doctrines advanced by Brown (Logic, vol. i., p. 224) and Hamilton. (Exam. of Hamilton, pp. 411, *seqq.*) Even Mr. Mansel is not exempt from this confusion, (Prolegomena, p. 213,) in spite of his distinction between intuition and thought. Let me add, that if any such complex notion is formed, it does not become the subject, but the predicate of the judgment. I do not judge that All heavy-air is heavy, but that All air is heavy-air—a judgment which is not only synthetical, but is merely the fuller expression of what I originally judged.

4. *Some Remarks on Mr. Abbott's Theory.*

Mr. Abbott would, perhaps, rather maintain that we see (originally) the difference of the distances from the eye of two objects than the actual distance of either of them. (See p. 27 of this Essay.) But this supposes that the real external objects are immediately perceived, since I presume it will be granted that we cannot see the differences of distance of two things, unless we see the things themselves. This doctrine has, therefore, I think, been sufficiently refuted in the foregoing pages. Indeed, the mere change of size in the apparent object, as we move away from it, affords a sufficient refutation, as Hume observed long ago. If Mr. Abbott thinks that what we see is not the difference of the two distances, but its "projection on the line of sight," this is only to make the theory more complicated without removing any of the difficulties.

Our author might also defend the veracity of our intuitive consciousness by asserting that the entoptic appearances, &c., whose apparent externality he insists upon, (Sight and Touch, p. 79,) were really external to the sensitive surface of the eye. But I do not think he could maintain that "the subjective appearances in the closed eye," or the "persistent impressions," (whose externality is confessed to be equally apparent,) are really external to the sensitive organ. Further, the same apparent externality, and even distance evidently belongs to the visual images presented in a dream, and I think also to those produced by other causes, such as pressure, electricity, inflammation of the nerve, derangement of the stomach, &c. In these latter instances there is, at all events, no visible object external to the retina, and there is no reason to believe that the visible appearance exists anywhere except on the sensitive surface. That it has a spacial cause I do not doubt; but since this cause is situated rather behind than before the retina, and bears no resemblance to the impression, it is impossible to regard it as the object seen. And here I may notice, that there is a certain ambiguity in the word "seen;" sometimes it is applied to the cause of vision, and sometimes of the visual appearance itself. If we take it in the former sense, the eye sees other things, but does not see itself; whereas, if we take it in the latter (which I think is the proper) sense, on my theory, the eye sees itself and nothing else. Nothing

I hold is seen perceived or intuited, except the retinal affection. But I do not think that mankind speak of all causes of vision as objects seen. They apply the term rather to one particular kind of cause—namely, that which causes vision by throwing light on the retina; and this for the reason that they usually believe that this object (and not the affection produced by it) is what we immediately perceive. But when the cause produces vision otherwise, (as is the case with electricity, disease, &c.;) and when there is no similarity between cause and effect, no one speaks of such a cause as *seen*. Even the vulgar would here apply the word "seen" to the affection itself, as I would do in all cases. To resume, then, the affection possesses the same apparent outness, and even distance in this case as in others; and, therefore, if outness and distance are equivalent to being outside of, and distant from, the retina, we are obliged (at least if we do not call in the Association Principle which would be fatal to Mr. Abbott) to regard direct consciousness as in this instance delusive. But on the explanations of outness and distance that I have given the delusion disappears; or rather it is resolved into another illusion, whose origin I have accounted for consistently with the veracity of our direct perceptions, viz., the illusion that there is an eye behind the collection of visual pictures. But this eye is not supposed to be immediately perceived—it is inferred in consequence of our mistaking the pictures of objects (especially of the members of our body) for the objects themselves. We know that the eye is behind these objects, and thence infer that it is behind the pictures of them; which inference we extend to *all* visual pictures, (or rather affections,) and are thus occasionally led into error.

Mr. Abbott lays much stress on the perception of depth (or relief) which arises when we combine two dissimilar pictures, and regard the object as one. It must be remembered, however, that the two pictures must be very nearly equal, similar, and similarly situated, or it will be found impossible to combine them. Further, I think that those who endeavour to combine them (especially when near the limit) will be conscious of a considerable effort in doing so—the two outlines having a tendency to separate from each other. This is more evident with the naked eye, as the reader will find, if he tries it on Mr. Abbott's figures. (Sight and Touch, p. 113.) It is not unreasonable to suppose that some change is effected in the shape

of one or both eyes by this effort, in consequence of which the two pictures really fall upon identical points, and that single vision in this case is therefore referable to the ordinary law. Why such double images suggest relief is evident. We have been ascertaining the existence of relief in all objects which require this effort to combine them from our earliest infancy, while we have never perceived it in those which are seen as one without any effort. I do not say that relief is suggested by the duplicity of these objects, since they are but rarely seen as double; but that is suggested by the effort which is required in order to see them single, and by their tendency to become confused at the boundaries.

The object of my quotation from Mr. Abbott, at p. 56 of the Essay, will, I think, be evident to the reader; but to avoid misapprehension, I may observe that my language, in describing the same facts, would be almost the converse of his. He accounts for angles less than a right angle appearing too large, because, in consequence of the spherical shape of the retina, (in passing I may remark that if we perceive the external objects and not the retina, I do not see how the shape of the latter could affect vision at all,) "the images received on it cannot be projected on a plane without distortion;" (Sight and Touch, p. 82;) while, in accounting for the same fact, I would say, that in consequence of the spherical shape of the retina, lines drawn on a plane cannot be projected *on it* without distortion. Without examining the mathematical phenomena of projection in detail, I may observe, that the three angles of a spherical triangle (taken together) are always greater than the three angles of a plane triangle; and, therefore, if (as I hold) the retinal picture is immediately perceived, angles in general will appear too large. I think a geometrical reason could also be given why this enlargement is greater in the case of acute angles. The area of a spherical triangle being proportional to the excess of its three angles over two right angles, the sum of the three angles of the external plane triangle, will be least augmented when it is so held that its projection on the retina shall be as small as possible. An extension of this remark will show why the illusions exhibited at p. 83 of Mr. Abbott's book disappear when we look along the lines. The retinal projection of any line exhibits fewest traces of sphericity when it is shortest, and most when it is longest.

The erectness or inverseness of vision, if satisfactorily ascertained,

would afford an *experimentum crucis* between my theory and that of Mr. Abbott. The reader will not confound my views with the current explanation of the (assumed) fact of erect vision, on the ground that when everything is inverted relative position remains the same. This, it has been justly observed, does not account for our seeing things erect, but merely for the inversion of the pictures (which is requisite for erect vision) escaping our notice. But instead of attempting to *explain* the universally assumed fact of erect vision, I *deny* it altogether; and as Mr. Abbott's theory affirms it, we are here directly at issue. I hold that if we could intuite the real external objects, we would find them turned in the opposite direction from what we now suppose, and likewise enormously larger. In proof of the inversion, I think Scheiner's experiments are conclusive. An illuminated point being held so near the eye that it produced the impression of a confused circle was looked at through three pin-holes in a piece of card. Three images of the point (the head of a pin held near a candle will do) were thus seen, and when the left pin-hole was covered, the right image or picture disappeared, and *vice versa*. A similar experiment may be made with one pin-hole, when, if we move the card up or down to right or left, the luminous point seems to move in the opposite direction. Another experiment, I think, confirms the same conclusion. If, with my eye open, I press the outside of the eye-ball, (next the cheek-bone,) a luminous ring appears which undoubtedly belongs to the impressed part; but it seems to be at the inside of the field of vision next the nose. On pressing the inside of the eye-ball the effect is reversed. If these observations are conclusive, Mr. Abbott's theory is refuted.

As persons couched for cataract are always deprived of most of the means by which Mr. Abbott thinks we naturally perceive distance and relief, (see Sight and Touch, p. 140,) the fact that they appear to be devoid of these perceptions immediately after the operation, cannot be regarded as a refutation of his theory. I hardly think, however, that the evidence of the fact is fairly stated in his tenth chapter. He argues, for instance, (page 147,) that Cheselden's patient must have seen relief in solid objects before he noticed it in paintings; for "if pictures had never (before this time) conveyed the idea of solid figures, it is clear that solid figures

were not seen as plain pictures." But Cheselden's patient had recognized his father's likeness in a locket long before, and these pictures were probably landscapes, the various objects in which it took him some time to familiarize himself with. Again, in describing Franz's case, (page 158,) he lays much stress on the fact, that the patient noticed at first sight that the sphere and the cube presented to him "were not drawings," while he notices but very slightly his repeated statements as to the flatness of all the objects which he saw; and it should not be forgotten that he failed to distinguish the sphere and cube from circular and square discs. This patient, as I have already observed, could see, previously to couching, a small patch of light through a lateral cleft in the capsule; and, therefore, when the light was suffered to fall on the whole retina instead of on this patch, it is not surprising that he decribed his sensation as "an extensive field of light," or that objects appeared larger than before. Mr. Abbott again calls attention to certain patients placing their hands close to the eye and then moving them out in straight lines toward the objects; but he does not observe that others continued groping about the eye for something at a distance from it, or that Mr. Wardrop's patient often placed her hand beyond the object and then moved it inwards toward the eye. Franz's patient also continued groping at the surface of a vessel of water for a ball sunk several inches beneath it. With these qualifications, I think, the cases reported by Mr. Abbott will be found in perfect agreement with my views. There can be do doubt that the patients afterwards came to see distance and (in some degree at least) relief; and the fact that their visual perceptions improved in this respect proves that association had something to do with the process; whether, as I think, by connecting different visual impressions with each other, or, as the Berkeleians maintain, by connecting visual with tactual and muscular sensations.* The incident of the pictures would alone

* Having touched again on the common theory, I may here make a remark which I omitted to make in its proper place. Even supposing that our present visual perceptions arise from an association between the original sensations of sight and those of touch and the muscles, the current doctrine seems needlessly complicated. A double association is postulated by Messrs. Mill and Bain; first, between the visual sensations and the muscular sensations which accompany the motions of the eye; and, secondly, between these mus-

suffice to establish this. But if distance from the eye and relief are acquired perceptions in these instances, the natural presumption is that they are so in all.

I should have wished to include some remarks on Mr. Levy's book on Blindness and the Blind, in the present volume, but the work did not appear until it was almost through the press. I find in it a strong confirmation of my view, that names may be used by different persons with the same denotation, but with different connotations. It should also be noticed that ordinary blindness is not total insensibility to light; and blind men, by close attention to their faint visual perceptions, may turn them to much better account than we should do. This is, perhaps, the explanation of "facial perception," which at first sight seems rather startling. At all events it gives no countenance to the modern theory, which lays stress on the muscular sensibility rather than simple touch.

cular sensations and those arising from the movements of the arm or the leg. Would not the theory be simplified by leaving out the intermediate link, as indeed was done by Berkeley? What is there to prevent our visual sensations becoming directly associated with our muscular efforts in moving our arms or legs? The true answer is, perhaps, as follows:—sight appeared to afford more information about objects than the theory would account for, and then a happy thought occurred to the theorists. When the eye is at rest, it was said, it does not see space; and its motions can only alter this perception by introducing associations with muscular sensations. The latter statement, however, which is rather assumed than asserted, is quite untrue.

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